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AN APPRAISAL OF SOME METHODOLOGICAL ASPECTS OF THE KINSEY REPORT*

PAUL WALLIN†

Stanford University

THIS article appraises two central aspects of the methodology of *Sexual Behavior in the Human Male*¹ by Kinsey, Pomeroy and Martin—a detailed account of a research project under way since 1938 and expected to continue for many years.

The project is a large-scale quantitative investigation in one of the most important areas of human experience, which until now has been studied largely by clinicians working with small, highly selected samples. From the volume under discussion, which submits findings on the sexual histories of 5,300 white males, it is clear that Kinsey and his associates already have collected a far richer series of quantitative data than any previous investigators. Their task has been difficult and they are to be congratulated for the courage and ingenuity with which they have prosecuted their research.

It is to be hoped that nothing will prevent their reaching the ultimate goal of 100,000 case histories to represent all segments of the country's population. In purpose and scope the project is one of the most significant studies of human behavior ever attempted and is deserving of universal support.

The shortcomings of the Report, the first major publication to emerge from the research, inhere largely in its failure to recognize or to make explicit the wide gap between the authors' aspiration and achievement to the present. Although they are highly critical of preceding studies, they sometimes minimize and occasionally completely overlook serious deficiencies in their own. This is the outstanding failing of the volume. Because of it many of the conclusions can easily be mistaken for firm generalizations rather than the provisional findings which at best they are. This evaluation is here supported by (a) analysis of the selection and composition of the sample on which the Report is based and (b) assessment of the evidence for accuracy of the data.

To orient the reader who may not be familiar with the Report, a brief description will be given of the major types of data which will be referred to in this article. No

* Manuscript received February 11, 1949.

† I am indebted to Professors Quinn McNemar and Lewis M. Terman for reading the manuscript and making valuable suggestions.

¹ Alfred C. Kinsey, Wardell B. Pomeroy and Clyde E. Martin, *Sexual Behavior in the Human Male* (Philadelphia: W. B. Saunders Company, 1948). From this point on the volume will be referred to as the Report. The senior author's name will be used occasionally for reference to the three authors.

summary of the findings or discussion of their implications will be offered. To do so would be to impute to its conclusions a degree of demonstrable validity hardly yet justified in view of considerations presented below.

Description of Principal Types of Sex Data

The data were obtained in interviews lasting from one to two hours. The bulk of the data in the Report are the responses of white males to questions on the incidence and frequency of orgasm in (1) nocturnal emission, (2) masturbation, (3) heterosexual petting, (4) heterosexual intercourse (pre-marital, marital, extramarital, post-marital), (5) homosexual contact, and (6) contact with animals. These sources of orgasm are called sexual outlets.

"Incidence" refers simply to whether orgasm was *ever* had in a particular outlet. The Report presents percentages on "active" and "accumulative" incidence. The former are the percentages of persons who have had orgasm in given outlets in specified five-year periods. Accumulative incidence is an estimate² of the percentage who will have had orgasm in the outlet by a designated age. "Frequency" refers to the *average frequency per week in a given five-year period* that orgasm was experienced in an outlet. Subjects were asked for incidence and frequency information for the years from adolescence to fifteen and for subsequent five-year periods up to the time they were interviewed. Frequency statistics are generally presented for the "total population" and for the "active population." In any one sub-group of the sample (e.g., single college males of the age period 21-25) the total population is all such persons in the sample. The active population consists only of those who have ever had orgasm in the period in question for the reported outlet. The active population is therefore usually appreciably smaller in size than the total population and markedly so for the homosexual and animal outlets.

² It is an estimate because allowance is made for persons whose histories at the time the data are gathered are not yet complete with respect to possible experience with the outlet in question.

Frequency data for both populations are given in terms of the mean (with standard error) and the median. The proportion of the total sexual activity (of the total and active population of sub-groups) which is derived from each outlet is also reported.

The Sample

1. Few, if any, researches depending on volunteer subjects have defined the scope of the universe to which their generalizations were to apply as ambitiously as the Kinsey project. In their summary of published quantitative studies on sex the authors classify the findings of half of them as inapplicable beyond their obtained groups of subjects, and the findings of the others as valid at the most for one or two sub-groups of the country's population. It is rather startling then to note that they believe their own findings can be extended to the "... 163 groups on which data are given" (p. 29). That this assertion is intended to be literally interpreted is attested by the fact that Chapter 23 presents in what are called "Clinical Tables" the incidence and frequency of the different outlets in each of the 163 groups.

These groups result from the successive breakdown of a sample of 5,300 American and Canadian white males by education (0-8, 9-12, 13+), marital status (single, married, formerly married, i.e., divorced, separated or widowed), residence (urban, rural),³ religious affiliation (Protestant, Catholic, Jewish), religious activity (active or inactive) and age (adolescence to 15, 16-20, and so on by five-year periods up to age 60). The division of the total sample by these sub-categories yields far more than 163 groupings of American and Canadian white males but data are not reported for all possible groups since some of them were not represented in the sample in what the authors regarded as sufficient number. Nevertheless one may well marvel at a sample of only 5,300 subjects which embraces 163 universes. In part this was accomplished

³ Persons who had "... lived on operating farms for an appreciable portion of the time between ages 12 and 18" (p. 682) were classified as rural, and all others as urban.

by the use of the histories obtained from older persons to provide data for all their preceding five-year age periods, the same subjects thereby serving as cases in a number of different age classes. Presumably histories of married subjects were similarly used to provide cases for both the single and the married groupings. Even though exception can be taken to this procedure of multiplying cases (as will be shown later), the sampling of the Kinsey investigation would still be notable if it had actually resulted in a selection of subjects which permitted generalization of findings for 163 groups. This would have required securing subjects representative of these many and varied universes, but the Report fails to demonstrate that this was done for even a small proportion of the groups about whose sexual behavior the authors generalize.

2. The most apparent limitation of the total sample and accordingly of each of the sub-group samples is in regional coverage. The large majority of the subjects were from the northeastern section of the country. The authors point out that "large sections of the country are not yet covered by the survey although it is certain that there are striking geographic differences in patterns of sexual behavior" (p. 6). But despite the admittedly incomplete regional coverage of the survey and the existence of "striking geographic differences" they have calculated national statistics for the total and active population of the country as a whole from the findings for their sample. This was done by weighting the raw data from each of the sub-groups in the study ". . . in proportion to the size of that group in the U. S. Census, and totalling the weighted results for all the groups" (p. 105). It is hardly necessary to dwell on the uncertain status of such national statistics. Any estimate of the frequency or incidence of a given phenomenon in the country as a whole can be no better than the sample from which it is derived. The U.S. statistics calculated for the Report are of dubious value if for no other reason than that it is impossible to even guess at the magnitude of their error in the absence of data for the unrepresented regions. Further-

more, as will be shown later, many of the statistics for the active population of the country are based on such small sub-groups in the sample that they could be right only by sheerest chance.

3. "The validity of extending generalizations derived from a study of any sample depends fundamentally and unavoidably, upon the representativeness of that sample" (p. 33). Since the Report generalizes about a large number of groups the question is whether—apart from regional representativeness—the total sample is composed of sub-samples of subjects representative of these groups. Securing a representative sample is not easy under the most favorable circumstances. Difficulties are considerably multiplied when the investigation depends on volunteer subjects and when participation requires them to be interviewed for an hour and a half or two hours on a phase of their lives which their culture conditions them to regard as nobody's business but their own. These were some of the problems Kinsey and his associates had to cope with in securing their sample and the evidence is against their having succeeded.

It is significant that the Report offers no estimate of the number of persons whose participation in the survey was unsuccessfully solicited through individual contacts or appeals to groups. That the number of non-volunteers was considerable is indicated by the fact that "Perhaps 50,000 persons have heard about the research through lectures and perhaps half of the histories now in hand have come in consequence of such contacts" (p. 38). The authors' comment on another study is applicable to their own. They say it is ". . . open to the very severe criticism that it involved only a highly selected sample of the total population. What is more serious, one is left guessing as to the histories of the 51 per cent that failed to answer the questionnaire" (p. 619).

The Report points out, only to dismiss, two factors which in all likelihood biased the sample. First, an unspecified proportion of the subjects volunteered their histories because of ". . . the opportunity to obtain in-

formation about some item affecting their personal lives, their marriages, their families, friends, or social relations" (p. 37). Some of the questions raised by the subjects concerned the following: possible harmful effects from "excessive" sexual activity, physical harm resulting from masturbation, sexual normality, problems arising from homosexual activity, information about available medical, psychiatric, or other clinics to which persons with special problems could be referred, etc. The Report states that "In a number of communities, public knowledge of this source of help has brought many histories" (p. 37). But the authors were not disturbed by this selection of many of the histories. To quote, "This does not mean that an undue number of neurotic or psychotic individuals has contributed. On the contrary, items of the sort listed above are the everyday sexual problems of the average individual; and the greatly disturbed type of person who goes to psychiatric clinics has been relatively rare in our sample" (p. 37). Granting the infrequency in the sample of the "greatly disturbed type of person," the research doubtless attracted subjects having sexual problems of one kind or another. The authors are patently begging the question when they characterize the problems of their subjects as those of the average individual since their conception of the sex life of the average individual is necessarily derived from persons selected by their research.

A second probable source of bias was the monetary payment given some subjects. The authors fail to indicate the number of persons paid for their histories but state that payment was ". . . confined to the economically poorer elements in the population, to persons who are professionally involved in sexual activities (as prostitutes, pimps, exhibitionists, etc.) or to others who have turned from their regular occupation and spent considerable time in helping make contacts. The payment has never been large, rarely amounting to more than a dollar or two for the couple of hours involved in contributing; and equivalent amounts may be paid to persons who have helped make the

contacts" (pp. 40-41). The authors may be right in their judgment that "payment did not distort the *quality* (italics ours) of the records" but they overlook the possibility that the promise of even a small financial return may have attracted subjects not typical of the lower income group. This could be checked by comparing the findings for paid and unpaid subjects from this group.

An unspecified portion of the sample was secured through "contact" persons. That these persons may have tended to weight the sample with sexually more active subjects is suggested by the observation that they ". . . have had to be educated to understand that 'a good history' is a history that accurately reports everything rather than a history that has some special element in it. Especially at lower levels, where the contact men have been paid, it was difficult at first for them to understand that the forty-minute history of an inexperienced teen-ager is as important as the two- or three-hour history of an older person who has been involved in every conceivable sort of sexual activity" (p. 103). That some self-selection of sexually more active subjects took place can be inferred from the statement that "The restrained histories have, on the whole, been the more difficult to get, and it has been constantly necessary to reassure individuals with relatively inactive histories that they were contributing to the study in as important a way as the persons with more active histories" (p. 103).

4. Judging representativeness of the sample is made unnecessarily difficult by the omission of a direct statement as to the source, size and composition of each of its constituent sub-groups. For reasons discussed later this is particularly serious with respect to the age distribution of the various sub-groups at the time of interview.

It is likely that representativeness is more closely approximated by the college segment of the sample than by the subjects of grammar and high school level. The greater accessibility of college males as research subjects increases the possibility of a diversified selection. Moreover, college males constitute a numerically smaller universe for

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sampling purposes than do those of lower educational levels. And since the former make up about 60 per cent of the Kinsey sample (p. 364), it may be that they are approximately representative of the college level male in the northeastern section of the country. That they are by no means entirely so can be deduced from comparison of data secured from hundred per cent and partial samples of college level men. The former are *all* persons in a group such as a fraternity or church organization. Partial samples are composed of individual volunteers and the incomplete membership of some groups. The Report states that 26 per cent of 12,000 histories accumulated by the project have come from hundred per cent groups. It does not give the percentage of the 5,300 male subjects so obtained.

The comparison of data from the two types of samples is vitiated by the fact that the subjects of hundred per cent groups are compared with those of hundred per cent and partial groups combined. This, of course, has the effect of reducing whatever differences there may be between the two types of samples. Despite this spurious element in the comparison, the total and active population of the so-called "partial" sample is shown to have consistently higher weekly frequencies on total outlet, masturbation, pre-marital coitus and homosexual contact in the three early age periods to which the comparison is limited. The only exception is nocturnal emissions (p. 94). Frequency comparisons are not reported for "petting to climax" and "intercourse with prostitutes." Active incidence percentages of the two samples in the age periods adolescence-15, 16-20, and 21-25 are close with the exception of those for homosexual contact on which the "partial" group has markedly higher incidence in all three periods.

The findings from this test confirm the suspicion that extent of sexual activity was a selective factor in obtaining the sample of college males. Since a similar test of grammar and high school males was not possible because of their insufficient representation in hundred per cent groups there is no way of knowing how the sample's sub-

jects compare with relatively unselected persons from these levels.

The representativeness of the grammar and high school subjects is very doubtful. There is evidence which argues strongly for the presumption that the sample of the lower educational levels is biased by the disproportionate representation of deviant groups. For example, two of the hundred per cent group were inmates of penal institutions and in *one* such institution histories were contributed by 350 men⁴ (p. 129). Several hundred male prostitutes served as subjects (p. 216). Moreover, histories were secured from an unspecified number of feeble-minded persons. (One of the hundred per cent groups was a "mental institutional group" [p. 95] but whether these subjects were mentally deficient or insane is not indicated.) These are groups in which there is unquestionably a concentration of individuals at lower educational levels. Obviously no sample of 1,500 to 2,000 subjects could possibly be representative of the universe of less educated males when made up in substantial part of such deviants.

The question of representativeness likewise arises in regard to sub-groupings of the total sample by age, religion, marital status, occupation, and rural-urban status. Since some of the sub-groups (e.g., older age classes, active and inactive Catholics, orthodox Jews, the previously married, and certain occupational groups) are represented in the sample by very small numbers, given the manner of their selection it would be pure chance if they were representative. The Report says of the post-marital histories of the previously married that "The sample is too small to warrant analyses even by age . . ." (p. 294). Yet, statistics on post-marital outlets are cited throughout Chapter 8 ("Marital Status and Sexual Outlet") and in the Clinical Tables, the latter being in effect tables of norms.

5. The authors have spent a great deal of

⁴The authors point out that the sexual experience of these men while in prison were not used in the calculations of rates of various types of sexual practices. Their extra-prison sexual behavior, however, does figure in many of the reported statistics.

time on the pragmatic determination of the minimum size of samples necessary to obtain what they consider adequately stable measures of the incidence and frequency of the different sexual outlets in the groups they were studying. These pragmatic tests and the conclusions derived from them have been criticized by McNemar, who sums up his evaluation with the remark that ". . . ignorance of four elementary statistical principles renders worthless this elaborate effort to determine how large N should be for a sub-group."⁸

Putting aside the question of the soundness of the sampling experiments we note the following conclusions by the authors:

"In a few cases, samples of 50 give a good indication of the results that a large sample would give. However, such small samples have been used in the present volume only when they belong to series for which most of the points are established by relatively large samples" (p. 88).

"Samples of less than fifty cases have not been used for any of the calculations in this volume." (p. 88, italics ours)

It is almost incredible both in the light of these statements and of simple statistical common sense to find that hundreds of calculations in the Report are based on less than 50 cases, many on less than 10 and some on 3, 2 or 1. With some exceptions the calculations are those for the "active" segments of many sub-groups. Since the "active" population (i.e., those individuals who had any activity in a specified five-year period in a particular type of sexual outlet) in a sub-group is almost always a proportion of the "total population" of the sub-group the latter may consist of hundreds of cases and the former of 50 or considerably less. For example, Table 124 (p. 461) indicates there are 50 cases of rural males of educational level 9-12 in the age period 21-25. The "active" population, however, is only 6 per cent or 3 cases. For these 3 cases

⁸ Quoted in L. M. Terman, "Kinsey's 'Sexual Behavior in the Human Male': Some Comments and Criticisms," *Psychological Bulletin*, 45, September 1948, pp. 443-59.

the table presents the mean (with standard error) and the median frequency of animal contacts. Even stranger is the calculation for the same table of the mean (omitting the standard error) and median for a single individual (rural, education 13+, age 26-30).

The extent to which the statistics for active populations have been based on absurdly inadequate samples can be readily seen in the Clinical Tables of Chapter 23. These tables present measures for the active portions of many but not all of the sub-groups for which data are reported in preceding chapters. The tables state the N's for the total populations of sub-groups and the percentages which the active segments constitute of the total populations. If the N's for the active segments had been tabulated in absolute numbers the error in the procedure would be more apparent.

The most astounding use made of small N's was as bases for calculating various statistics for the active population of the entire country. For example, Table 58 (p. 258) reports the mean frequency of the homosexual outlet and the percentage it constitutes of the total outlet for active married males in the United States in different age groups. In all but one instance these national statistics are based on sample N's of less than 30.

The authors' own assumptions as to size of groups needed for reliable sampling require that their findings for active populations of many of the sub-groups be dismissed as valueless. This should be borne in mind in considering many of the tables in the Report.

Evidence for Accuracy of the Data

Evidence of their accuracy is especially essential for the data of this study for a number of reasons. First, most of the data are men's reports of past events in their histories. A large proportion of the information obtained from older males was recollections of their sexual behavior ten, twenty or thirty years before and may have been greatly affected by error of recall.

Second, the data on frequency of orgasm in the various outlets are estimates *per week*

per five-year period, and as the authors point out "... it is difficult for a person who has not kept a diary, and especially for one who is not accustomed to thinking in statistical terms [and most persons are not], to know how to average events which occur as irregularly as sexual activities usually do" (p. 120). And again, "Estimates of average frequencies are especially difficult for children, for individuals of low mentality, and for most poorly educated individuals" (p. 149).

Third, accounts of experience in the various outlets may be influenced by subjects' attitudes toward them. As the Report points out, evaluations of the different outlets as normal or abnormal, moral or immoral, undoubtedly differ among sub-groups in our society. An obvious case in point is the difference between devout and non-devout Catholics in their evaluation of extramarital intercourse; another is the influence of education in determining persons' conceptions of masturbation and nocturnal emissions as normal or abnormal, since the better educated are exposed to literature which emphasizes the normality and near-universality of these outlets. There are also probably group differences in prevailing stereotypes with respect to the relation between various kinds of sexual activity and masculinity-virility. In some groups male self-esteem may be thought to require intercourse with numerous females as proof of virility. Conversely, group norms may define masturbation and nocturnal emission as negative indications of manhood. Depending on their nature, both types of group standards could lead subjects, consciously or otherwise, to underestimate experience in some outlets and to overestimate in others. The constant errors thus introduced could completely invalidate the statistics in many inter-group comparisons.

Since the conditions discussed above pertain to most of the data in the Report, evidence on the accuracy of the data is of paramount importance. The evidence presented by the authors can be classified roughly as (a) intuitive-impressionistic and (b) objective.

(a) *Intuitive-Impressionistic Evidence*

1. The authors assume that the use of the interview as opposed to the questionnaire increases the accuracy of the data secured. ("The most serious error in these studies [earlier quantitative studies of sexual behavior] has been the wide use of questionnaires") [p. 31]. The only evidence offered in support of this assumption is the fact that a greater number of persons in the Kinsey survey than in any questionnaire study have admitted involvement in socially taboo types of sexual activity (p. 43). That this difference may have been determined by a selected bias in the sample of the former, an equally possible and plausible explanation, is overlooked. It is still an open question whether individuals will report socially taboo behavior more frankly in an anonymous questionnaire or in an interview which requires them to reveal their identity and to speak of their experiences in a face to face relation with the investigator.⁶

2. The authors assert that one check on deliberate falsification and exaggeration by subjects was the procedure of "Looking an individual squarely in the eye and firing questions at him with maximum speed . . ." (p. 54). This method which requires subjects to answer questions with little or no opportunity for premeditation, is characterized as "... one of the most effective checks on fabrication, as detectives and other law-enforcement officials well know" (p. 54).

"Firing questions" with maximum speed may elicit rapid replies but one cannot help wonder how older subjects, particularly, could estimate in a few seconds the average frequencies of the different sexual outlets for five-year periods ten or twenty years

⁶ Two published studies of the problem failed to establish the superiority of the interview in this regard. See Albert Ellis, "Questionnaire versus Interview Methods in the Study of Human Love Relationships," *American Sociological Review*, 1947, XII, 541-53 and the same journal, 1948, XIII, 61-65, and Edith S. McCall, Appendix to Katherine B. Davis, *Factors in the Sex Life of 2200 Women*, New York: Harper, 1929. The Kinsey Report refers to a test of the comparative value of the interview and the questionnaire in the first year of the study, and the neither data nor findings are given (p. 11).

past. If the method was used for frequency data one might well be suspicious of responses given without considerable deliberation. Moreover, a detective-like cross-examination of subjects would seem to be incompatible in spirit and objective with the informal character of the interview as described in a preceding section of the volume ("The subject should be treated as a friend or a guest in one's own home") [p. 48].

In any case, the procedure in question has no demonstrated efficacy as a check on fabrication of statements made in the context of a research interview situation. The impressionistic judgment of Kinsey and his fellow interviewers may or may not be correct.

3. The authors were clearly aware of the danger of leading questions biasing replies. They point out that "In his tone of voice and in his choice of words, the interviewer must avoid giving the subject any clue as to the answers he expects" (pp. 52-53). But this sound principle is violated by their practice of "placing the burden of denial on the subject." ("We always assume that everyone has engaged in every type of activity. Consequently we always begin by asking *when* they first engaged in such activity. This places a heavier burden on the individual who is inclined to deny his experience . . .") [p. 53]. Although this approach may reduce the tendency to fabrication in some individuals the potential gain in accuracy of data in these instances could be more than counterbalanced by the effect of the method on two classes of subjects. First, it could—as the authors recognize—bias the responses of highly suggestible individuals. Second, it could encourage what for some persons are the more prestigious answers. For example, some unmarried males might be ashamed to admit never having had sexual relations. Asking them when they first had intercourse thus would make it easier for them to avoid confessing their "inadequacy." The authors state that ". . . there is no indication that we get false admissions of participation in forms of sexual behavior in which the subject was not actually involved" (pp. 53-54). Unfortunately,

they give no objective evidence in support of their opinion.

4. The authors believe that an effective control on subjects' "cover-up" of tabooed items in their histories is the introduction of a series of interlocking questions. Before a direct question is put about a particular sexual experience, a number of indirect preliminary questions are asked, the answers to which presumably make it difficult for subjects to deny the experience if they have had it. The preliminary questions—there were twelve concerning homosexual experience in each interview—for the various sexual outlets are (like the forms of all questions in the interview) not given in the Report, making it difficult to evaluate the method. A second check on "cover-up" is said to be the use by the interviewer of vocabularies unique to persons with specific types of experience. Thus a prostitute would betray her vocation by responding with understanding to the argot of her group.

Even if there were no question about the wisdom of the techniques discussed above and others employed in the Kinsey study there is no basis other than the judgment of the interviewers for estimating whether they attained their purpose. Safer measures of the *achieved* accuracy of the data must be looked for in the more objective, quantitative evidence to which we now turn.

(b) *Objective Evidence*

1. One objective test of the data to which the authors attach great significance is a comparison of the information obtained in re-takes of whole histories from 162 subjects, after an average interval of 38.5 months. This is not a test of the accuracy with which subjects' reports represent their actual experiences. Although included in the chapter on "Validity of the Data," it is rather, as the authors point out, a test of consistency of report. Consequently, a finding of consistency between the original and retake histories could be due in large part to the stability of false or inaccurate recollections. On the other hand, inconsistency would suggest that the data being sought cannot be accurately reported by the sub-

jects. The evidence of witnesses whose testimony varies from one time to another is of questionable accuracy.

No indication is given of the circumstances under which the subjects of this test were procured for re-take histories. This information is important for an appraisal of the worth of the test. There is some evidence that marked sexual activity in certain outlets was a selective factor for participation in the test. The incidence of homosexual experience in this group is almost 50 per cent, which is considerably higher than the incidence reported for the population at large from which the group is drawn.

For some unexplained reason the test is reported for 108 males and 54 females and the comparisons of the original and re-take histories are made with the responses of men and women combined. Since it is the accuracy of male subjects which is at issue this makes the test of dubious value unless it be assumed that the memory constancy of male and female is identical for sexual events in their histories, a question not even raised by the authors.

The findings for the men and women are given in Table 13 (pp. 122-23). One notes immediately a feature of the table characteristic of many other tables in the volume; the size of the group for which item comparisons are made varies markedly. For example, only 104 (of a possible total of 162) subjects are involved in the comparison of the original and re-take histories with respect to the item on whether they had ever had an orgasm in a homosexual contact. Two possible explanations of the discrepancy suggest themselves. First, the information in question may not have been sought in the earlier period of the study and therefore would be absent from the original histories of some of the subjects. This seems unlikely for such a critical item. Second, some of the subjects may have failed to answer the question in one or both of the interviews. If this accounts for the missing cases, their omission from the reported analysis is a serious methodological error.

Similarly one is left to guess at the reason for incompleteness of the data in compari-

sions of other items. While incidence of homosexual outlet is indicated for 104 subjects, frequency is reported for only 84. Rural-urban status, religious grouping, devoutness, etc., are given for less than two-thirds of the total group.

Another inadequacy of the table is in the presentation of data on frequencies of the various sexual outlets. Since the subjects averaged about 25 years of age when first interviewed, frequency estimates were presumably secured for the years from adolescence to 15, 16-20, and 21-25. However, the time span covered by the frequencies reported in the table is not specified. (A valuable test could have been made by comparing the relative consistency of frequency information about the earlier and later periods.) Active incidence figures are likewise not reported by age periods. As given, the data indicate test-retest reliability is high on incidence items but far below acceptable standards on frequency items. Twelve⁷ of the 47 items for which test-retest consistency is reported do not figure in the data of this volume and hence are irrelevant for the purpose at hand.

2. A second objective test of some of the data was made by comparing information secured from the husbands and wives in 231 couples. Here the validity of the test hinges in large part on whether there was any collusion between the partners, since they were interviewed at different times. The authors point out that allowance must be made for the possibility that collusion occurred, but unfortunately there is no way of estimating *how much* this allowance should be.

The comparison of the data of husbands and wives is shown in Table 14 (p. 126). The correspondence between couple members is reported for 32 items. None of these relates to incidence of sexual outlets. Four items involve frequency reports and these are with respect to marital coitus. Only two of these can be said to roughly parallel the frequency items of the Report: "average fre-

⁷ Ages of father and mother, number of brothers and sisters, and age at first knowledge of pregnancy, coitus, fertilization, menstruation, abortion, prostitution, venereal disease, condoms.

quency in early marriage" and "average frequency now." Nonetheless, in speaking of the correspondence between responses of spouses on the 32 items the authors say, "The coefficients of correlation and the percentages of identical replies are low *only* [italics ours] in regard to the frequencies of marital coitus, and in regard to the percentage of the time in which the female reaches orgasm during the marital coitus" (p. 127). Yet these were the sole frequency items tested and the average correlation between husband-wife replies to them was only .60. Since frequency and incidence data constitute the bulk of the Report, the reader could easily be misled by the authors' conclusion that "For most items of the sort covered in this study, it may be expected that something between 80 and 99 per cent of the subjects will give replies that will be verified, independently, by the partners in their marriages" (p. 128).

Two additional circumstances limit the value of this and the previous test. First, there is the fact that the groups used in the tests as well as in others still to be mentioned were made up almost entirely of persons at the college level. There is virtually no precise evidence on the accuracy of data secured from subjects at grammar school and high school level. The authors point out, however, that "Preliminary examinations of data from lower social levels suggest that variations in the quality of such reports are wider than the variations among college males" (p. 153). Secondly, the average age of the males in the first test was about 25, in the second test about 33. On any items involving recall the findings of the tests therefore cannot be generalized to males in older age groups recollecting more distant experiences.

3. As a test of the accuracy of one item, the authors compare the data on age at onset of growth of pubic hair drawn from three direct *observational* studies with those reported by their own subjects on the basis of *recall*. The age distributions are found to be very similar (Table 15 and Figure 15, pp. 130-131). It is regrettable that the Report fails to give the ages of the Kinsey subjects at the time of recall. If the majority

involved in the comparison were in their late teens or early twenties the correspondence of the data obtained from them with those of observational studies reveals little or nothing about the accuracy of recall of persons in their thirties or forties which is especially open to question. The authors failed to take advantage here of the opportunity to test the relative accuracy of recall of persons in different age groups. The same might have been done for persons of different educational levels, but the educational composition of the group is also not reported.

No explanation is given as to why 2,511 persons, somewhat less than half of the total sample of 5,300 subjects, are represented in the comparison reported above. As pointed out earlier, this question of why eligible subjects in varying number are omitted from table to table comes up persistently throughout the Report.

4a. As a test of memory of recent compared with remote events the Report cites the essential similarity of data secured from members of the older and younger halves of the Kinsey sample. The younger generation is reporting more immediate experience, the older generation is recalling the more remote experience of early years. Apart from the fact that many significant differences are revealed in the comparison of the sexual practices of the two groups (see Tables 104 and 105, pp. 410 and 412), the argument is entirely specious. The comparison of the generations is legitimate only if it be assumed that immediate and remote recall are equally valid. Findings contingent on this assumption cannot be used in turn as evidence of its soundness.

4b. As stated earlier, evidence on accuracy of reporting remote events is important for the Kinsey study because many of its data are recollections by older males of the sexual experiences of earlier age periods. These data are combined in the volume with the reports by younger males about more recent experiences in the same age periods. Two conditions must be satisfied if statistics derived from combining the two series of data are to yield an approximate representa-

tion of sexual experiences at the younger age levels. First, the two series must have about the same degree of accuracy. Second, it must be demonstrated that no appreciable change has taken place in sexual practices in the age-periods being studied since the older males were in the age-periods in question. The Report fails to show that either condition has been satisfied.

5. The Report considers two significant questions which arise in research based on interview materials. The questions are (a) whether different interviewers get corresponding findings from comparable groups of subjects and (b) whether one interviewer secures relatively similar findings from successive groups. In both cases evidence of consistency of findings is necessary but insufficient proof of accuracy of data, since consistency could be achieved despite constant errors in interview procedure and in the reporting of the interviewees.

The Report compares (a) data obtained in the years 1943-46 by the three authors and (b) data obtained by Kinsey in his interviews in this period with those of his 1938-42 interviews. Comparisons are made on active and accumulative incidence, and on mean and median frequencies of total sexual outlet, masturbation, nocturnal emission, pre-marital coitus and homosexual activities. The omission of petting to climax, intercourse with prostitutes and animal outlet is not explained. The age periods covered are 11-15, 16-20 and 21-25.

The subjects in the analysis were of similar race, marital status, and age. All were of college level. In addition, two of the interviewers are compared on accumulative incidence data secured from persons of grammar school level. This comparison is limited to total intercourse and intercourse with prostitutes. Although the data are reported in tabular form no mention is made of them in the text. Neither active incidence nor frequency data are presented. In view of the dearth of material for appraising information given by high school or grammar school subjects the omitted comparisons would have been of considerable interest to the reader.

The data obtained in the years 1943-46 by the three interviewers (p. 134) show close agreement on accumulative and active incidence data. (It is not clear why, in the comparison of accumulative incidence data, the Kinsey group consists of his 1943-46 interviews combined with those of 1938-42, whereas in all other comparisons only the later interviews are used.) Differences between *median* frequencies are in the main small and are not consistently higher or lower for any one interviewer. The Kinsey interviewees, however, have the highest *mean* frequencies on 28 of 30 sets of comparisons. On many of these Kinsey's means are significantly in excess of those of one or both his fellow interviewers. Kinsey's higher means could be due, as the Report suggests, to some selection in assignment to him of more promiscuous and difficult subjects. In the light of this explanation the differences between Kinsey and the other interviewers are in general in the expected direction. The extent of the differences, however, cannot be evaluated since it is unknown how much selection determined the composition of the Kinsey group.

The statistics for the Pomeroy and Martin subjects differ from each other appreciably only in regard to the mean frequency of the homosexual outlet. In both the 11-15 and 16-20 age periods Pomeroy's means are higher by several hundred per cent. This difference between the two interviewers could easily be due to sampling error.

If the latter explanation be assumed to account for the lower means of Martin's group on the homosexual outlet and the bias in Kinsey's group be accepted as the explanation of its higher means, the data of the three interviewers can be said to match fairly well.

The data secured by Kinsey in two successive periods (1938-42, 1943-46) on the whole correspond rather closely. There are no consistent differences between the statistics for the two periods (p. 142). This correspondence is somewhat surprising since on the basis of the previously indicated bias in the selection of Kinsey's 1943-46 subjects, it might have been expected that his

1938-42 group would score consistently lower on the various outlets. That this did not occur suggests that in the earlier period of the study there was an unwitting or uncontrolled selection of sexually more active subjects.

The results of the comparisons of the statistics of the three authors and of Kinsey in successive periods demonstrate a fair degree of consistency in the data and their processing. If the possibility of constant error in subjects' reporting or in interview procedure could be ruled out, the results of the comparisons could be said to certify to the accuracy of incidence and frequency information obtained from college level males of predominantly younger age groups. In view of the qualifications which attach to the interpretation of the results of the test, the findings cannot be accepted without reservation. Nonetheless, of all the evidence adduced for data validity or reliability this appears to be the most satisfactory. It is unfortunate that due to paucity of cases comparable evidence is not available for older males in general or for subjects of grammar or high school level.

6. Many of the data on the different outlets are graphically presented and the authors state that while irregularly shaped curves representing data for successive age groups or educational levels are not necessarily inaccurate ". . . there is some warrant for considering that smooth trends in such curves are evidence of their approach to reality" (p. 132).

This argument for the accuracy of the data is a plausible one. Smooth trends can represent correlations between plotted series of data, and correlations are not likely to be found for data which are grossly inaccurate. Smooth curves, however, do not rule out the possibility of constant error in the data and hence correlations depicted by ascending or descending curves may be considerably higher or lower than they should be.

A large number of charts are cited as "striking instances" of smooth trends (p. 132). These cannot be reviewed in detail, but they merit some general comment. The most strikingly smooth trends are those

showing the accumulative incidence from age 5 or 10 to 45 of the various outlets among the grammar, high school and college level males. Empirical testing,⁸ however, reveals that the smoothness of accumulative incidence curves is an artifact of the way in which the accumulative incidence statistics are calculated. Those curves therefore have no bearing on the issue of data accuracy.

Another of the series of charts cited which are irrelevant to the argument are those showing the frequencies with which the outlets are used by males in the three educational groups in various age periods. These charts are merely graphic representations of simple frequency distributions which permit no inferences as to accuracy of the data.

A third set of charts to which the authors call attention are bar charts showing the mean frequencies of the outlets for single males in the age group 16 to 20. The bars indicate the relative frequencies of each outlet at the three educational levels and in seven occupational classes. Since educational level and occupational class are roughly correlated, only the charts for the former will be considered. Correlation between mean frequencies in the outlets and education would be indicated if the frequencies for the three educational levels were successively higher or lower. This does not occur in the case of marital intercourse, homosexual contact, and total sexual outlet. Nor does it occur in the case of animal intercourse and extra-marital intercourse with prostitutes for which bar charts are not presented. Petting to climax yields successively higher frequencies from the lower to higher educational levels but tabular data (p. 346) show the trend does not hold in the adolescence to 15 and 26 to 30 age periods. A trend does obtain for masturbation, nocturnal emission, total premarital intercourse and premarital intercourse with prostitutes.

⁸ Table 12 (p. 116) shows how accumulative incidence curves were calculated. In the test here referred to the cases in column 1 of the table were more or less randomly shifted to other class intervals. Despite the fictitious distribution thus obtained of age at first experience of coitus with prostitutes the data still yield a smooth curve.

A fourth series of charts mentioned as showing smooth trends are those in which mean and median frequencies of total outlet and of particular outlets are plotted against age for males of single, marital and post-marital status. These charts are for the total sample and for the country as a whole. Parallel charts are cited for the active population of the sample and the U.S. Since the sample is heavily weighted with college level males, the shapes of curves for the total sample are disproportionately determined by data obtained from college males. A second condition which limits the usefulness of this series of curves as evidence of data accuracy is that values of many points on the curves are not independent since individuals in older age groups are represented in the statistics for the younger age periods. Because this fictitiously reduces sampling error it spuriously increases the smoothness of the curves for both the total sample and for the United States. Consequently it is difficult to evaluate the evenness of trends in these curves. Many of the curve trends are not particularly smooth, and could represent data involving considerable inaccuracy.

The "smooth trend" evidence for accuracy of data in general is not as convincing as it might first appear. At most it is presumptive evidence for (a) the findings of differences on some of the outlets between the educational sub-groups of the sample and (b) the findings of some relationship in the sample between age and incidence and frequency of the various outlets.

7a. Although not bearing directly on data accuracy, two additional comments are pertinent for assessing the data. In connection with the problem of interviewer consistency the Report states that "One of the questions most frequently raised about the present research, and a thoroughly legitimate question about any research, concerns the possibility of another investigator duplicating the results." A serious criticism of the Report on this score is its omission of the wording even of questions used to elicit the major sex data. Despite the fact that the questions were never standardized in form, some knowledge of the variant forms used is

essential for repetition of any part of the Kinsey study.

7b. Frequent reference is made in the Report to attitudes of various sub-groups to different sexual outlets. It is usually impossible to determine whether conclusions about attitudes are based on some unpublished attitude data or are inferences drawn from the data on sexual outlets. Only one table in the volume presents any data on attitudes ("Attitudes on pre-marital intercourse at three educational levels," p. 364).

Conclusions

Given its subject matter and sampling scope it was to be expected that the Kinsey investigation would encounter great difficulties both in securing a representative sample and in getting accurate data. This article has examined in some detail the evidence of the extent to which these difficulties were met and overcome. The conclusions can now be drawn.

Whatever the merits of the findings, they are not safely applicable beyond the north-eastern section of the U.S. and no reliance can be placed on the Report's national statistics. The statistics for active populations of many sub-groups in the sample and for total populations in others are valueless because they were derived from too few cases. This applies particularly to statistics on the prostitute, homosexual and animal outlets. It is very likely that the sample as a whole was weighted with sexually more active males and that consequently the incidence and frequency statistics are considerably higher than would be obtained for more representative samples of the various sub-groups. The segment of the sample which is probably most nearly representative of its universe is the urban, Protestant group under 30 years of age with one or more years of college. The representativeness of the high school or grammar school subjects is very dubious as is that of the older males, Catholics, orthodox Jews, the previously married and members of certain occupational groups. The findings for these sub-groups should be regarded as very tentative.

The nature of the data dealt with in the

Report makes evidence of their accuracy particularly essential. The critical question about the data is not whether they "approach reality" but rather how closely they do so. Since all the subjects were volunteers, the large majority probably were prepared to give more or less truthful information about their sexual experiences. But this orientation is no assurance that their reports were not materially distorted by (a) their attitudes to the different outlets, (b) the necessity of estimating average frequencies for five-year periods and (c) incorrect recollection of events in past years.

It is difficult to validate accounts of sexual experience since checks external to the subjects are not readily available. The Report describes a number of devices used in the interview to reduce fabrication or concealment, but their effectiveness can only be guessed at. Findings from some of the more objective tests of the data are for various reasons irrelevant to the question of accuracy or of limited value. The evidence on data accuracy is more adequate for the younger males with some college education than for those at lower educational levels. Incidence information given by the former probably has rather high validity but frequency data in all likelihood have a substantial margin of error. The validity of information secured from the grammar school and high school subjects and from older males is argued for almost entirely by the fact that the various

statistics calculated for successive educational and age levels yield smooth trend curves. The trends in some of the curves referred to by the authors are artifacts and other curves are not pertinent to the argument. Many of the relevant curves are not notably smooth and at best indicate that the data are only grossly accurate.

In general it might be said that the evidence warrants acceptance of the finding that *in the sample studied* there are differences in regard to various sexual outlets among educational levels, age groups, religious groups, occupational groups, and rural-urban groups. The considerations advanced in this article dispose the writer to conclude that only the findings for the younger age groups of urban, Protestant, college level males can be generalized from the sample to the universe.

The phenomenal sale of the Report is well known. It is highly regrettable that only a negligible fraction of its readers will recognize its limitations. It should be emphasized that the Kinsey research program has been and is being carried forward in the face of enormous difficulties associated with large scale quantitative research in human sexual behavior. These difficulties need not be minimized and had the authors recognized and made explicit the present limitations of their sample and data, the Report might have been less sensational but would have gained considerably in scientific merit.

QUERIES CONCERNING INDUSTRY AND SOCIETY GROWING OUT OF STUDY OF ETHNIC RELATIONS IN INDUSTRY*

EVERETT C. HUGHES

University of Chicago

A NUMBER of sociologists about two decades ago focussed their studies of race relations on ethnic divisions of labor found in the colonial regions of the world which capitalistic industry had but recently penetrated.¹ More recently American sociologists have joined in study of race relations in industry in their own country with the aim of learning how we may make fuller use of the American labor force regardless of racial or ethnic distinction. I think it now profitable to draw these two perspectives together; and in so doing, to raise some general questions, not merely about the role of industry in mixing peoples, but concerning the relations between industry and society in various situations. For whenever one scratches a problem of racial and ethnic relations, he uncovers problems concerning society itself; and in this case, concerning industry and society.

First, let me make three sweeping statements which are germane to the whole problem of ethnic relations in industrial economies.

The first is that industry is always and everywhere a grand mixer of peoples. War

and trade mix them, too, but chiefly as precursors to the deeper revolutions of work and production in which the more fatal mixing occurs. In no considerable industrial region of the world has an indigenous population supplied the whole working force. Some of it comes from such a distance as to be noticeably different from the local people. The resulting ethnic differences within the industrial population may be small, as between the various parts of Great Britain, or as between Yankees and Southerners in this country; or great, as between Poles, eastern Germans and western Germans in the Ruhr and Rheinland, or as between English and French-Canadians in Quebec. They may be extreme, as between South African natives and Europeans, or between North Americans and native Indians of Peru. In short, the differences may range in magnitude from those between regions of the same country, through those between different European nationalities, to those between European and non-European, the latter being most extreme when some of the people are of tribal cultures whose institutions of property and work are entirely different from those known in Europe.

Industrial regions vary also as to the positions of local people relative to those of immigrants. At one pole are those regions in which the working force is built around a nucleus of native controlling and technical personnel and skilled workers, and where successive waves of immigrants enter the working force at the bottom of the skill hierarchy. At the other extreme are those regions in which the controlling and highly skilled nucleus goes out to establish industry in a remote, unindustrialized part of the world; the labor is recruited either from a native population, or—and this is common—is imported from still other ethnic areas.

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¹ See E. B. Reuter (Editor), *Race and Culture Contacts* (N.Y.: McGraw-Hill Co., 1934), in which papers of the 1933 meeting of the American Sociological Society appear; especially, R. D. McKenzie, "Industrial Expansion and the Interrelations of Peoples," and R. E. Park, "Race Relations and Certain Frontiers." See also McKenzie, "Cultural and Racial Differences as Bases for Human Symbiosis," in Kimball, Young, *Social Attitudes* (N.Y.: Henry Holt, 1931). For still earlier work on the problem, see Sidney Olivier, *White Capital and Coloured Labour* (London, 1906) and *The Anatomy of African Misery* (London, 1927); James Bryce, *The Relations between the Advanced and the Backward Races of Mankind* (London, 1902).

It thus may happen that practically the whole industrial personnel is alien to the region, while the various ranks are alien to each other, as in the mines and plantations of the Malay peninsula.

Finally, the industrial regions vary as to the kinds of industrial and social structures which develop and in the degrees of upward mobility possible for various racial and ethnic elements of the working force. Again, at one pole are the somewhat open structures in which there is a theoretical, although practically limited, possibility for a person of any ethnic kind to fill any position; at the other, rigid systems of stratification in which the people of each ethnic group are limited to a narrow range of jobs or ranks. Each of these kinds of variation has its accompaniments in the industrial community, and eventually in the political and social conflicts, alignments and movements which follow the development of industry.

My second sweeping statement is that modern industry, by virtue of being the great mixer, has inevitably been a colossal agent of racial, ethnic and religious segregation. For segregation, if it means something more than that isolation in which the peculiarities of race and culture develop, refers to some degree of functional separation of different kinds of people within a common system. Industry brings people together and sorts them out for various kinds of work; the sorting will, where the mixture is new, of necessity follow racial and ethnic lines. For cultures (and when races first meet they are always unlike in culture) differ in nothing more than in the skills, work habits and goals which they instil into the individual. These differences may tend to disappear in the course of industrial experience, although segregation may tend to keep them alive in some modified form for a long time. At any rate, there is not yet—even among the older industrial regions, where ethnic differences have been reduced by common experience and intermarriage—one in which one may not discern some deviation from chance expectation in the distribution of ethnic and religious groups among the various kinds of work and the

several ranks of industrial organizations.²

The third of the sweeping statements is that industry is almost universally an agent of racial and ethnic discrimination. There is no question about it if we take the word discrimination in its basic sense of the action of making distinctions. For those who hire industrial help must nearly always choose from among people who are not all alike ethnically, and very often from among ethnic groups whose industrial experience and training are far from equal. Furthermore, when industry actively seeks labor from new sources, it generally has to make an ethnic choice.

In sociological language, discrimination has taken another meaning: that those who pick people for jobs consider, intentionally or unwittingly, traits not directly relevant to work. If we accept this meaning, industry is still almost universally an agent of ethnic discrimination. In all industrial regions, again including the oldest, there is current among managers, foremen and industrial workers a body of opinion and lore concerning the work capacities and habits of various ethnic groups. Insofar as such belief and lore do not correspond to verifiable fact, they point to discrimination. Certainly they hinder clear perception of the differences between individuals of a given ethnic or racial group. In some industrial regions, discrimination is openly defended; in a few, enforced by law.

We have defined industrial segregation as deviation from chance in the distribution of people of various ethnic groups among the positions in industry. Discrimination we have defined as consideration of racial,

² This is so obviously true in North America as to need no proof. For evidence concerning western Germany see: Wilhelm Bepohl, *Der Aufbau des Ruhrvolkes in Zuge der Ost-West-Wanderung*, Recklinghausen, Bitter & Co., 1948. Everett C. Hughes, "The Industrial Revolution and the Catholic Movement in Germany," *Social Forces*, XIV (Dec., 1935). There are marked differences between the positions of Flemings and Walloons in Belgium; Protestant and Catholic in Holland; Flemings, Italians, etc., and French in France. It is commonly said that such differences of distribution still exist as between Welsh, Scotch, Irish, and English of various regions of the British Isles.

ethnic or religious traits in selection of workers even when the traits are not known to be relevant to work behavior. But segregation is not of itself evidence of discrimination. For there are undoubtedly cases in which even the most objective and sharp selection of workers by known or probable work performance would result in racial and ethnic segregation. On the other hand, we do not know how long it would take, under an aggressively objective policy, for all racial and ethnic disparities in job distribution to disappear. The truth is that no one has worked out a statistical device for establishing the existence or degree of ethnic discrimination. It would be very difficult to do so. For a given organization may have a great variety of jobs and positions, each of which has its own complex of activities and skills, and consequently of required training and experience. The positions have each their own rate of turn-over, so that they vary in their sensitivity to ethnic change in the labor supply. Past discrimination leaves its mark in varying degree and for varying length of time. But the lack of such an index need not worry students of the problem unduly. For discrimination is generally admitted, although it may be called by other names when discrimination becomes a bad word. In fact, the evidence of recent studies indicates that at least an unconscious discrimination tends to permeate industrial organizations even in the rare moments when conscious effort is made to avoid it.

It is an interesting and apparently paradoxical observation that modern capitalistic industry, which has developed a strong, sometimes ruthless ideology of indifference to persons, of choice of the best article for the purpose, and of the best man for the job, and which has shown a great drive, almost a mission, to sweep away beliefs, customs and institutions which stand in the way of industrial development, should also have become not merely—as one might have expected—an aggressive and grandiose mixer of peoples, but also a great and sometimes stubborn agent of racial and ethnic discrimination and a breeder of racial doctrines and stereotypes. This raises the general ques-

tion whether and under what circumstances modern industry is really guided by the impersonal concepts of the market and efficiency in choosing and assigning its labor force.

Another tenet of the ideology of modern industrial management has been that all barriers to free movement of labor should be removed in the interest of its economic use. This appeared in the movement to remove restrictions against internal migration in the early days of the industrial revolution in Europe, and in the insistence on treating each worker as an individual whose employment could be terminated at will by either party without interference of any third party. With reference to this tenet we may ask: To what extent and under what circumstances does industry rely on purely economic means and incentives applied to freely moving individuals to get and keep a labor supply, and under what circumstances does it use or encourage essentially political means, such as restriction of movement, fixed terms of employment, or differential rules governing the movement and activities of certain categories of people? There are other questions like these concerning the behavior of industrial management in various social settings. I believe it one of the major tasks of people interested in a sociological view of industry to seek the answers to these questions, and that the way to do it is to compare the ways of industry in a variety of settings, including a variety of inter-racial and inter-ethnic settings.

A first and evident comparison is that between the mother-countries of modern technology and industrial institutions, including their closer satellites, and the outlying, newer industrial regions which we may call colonial whether they are so in a political sense or not.

In the mother-countries (England, Belgium, Holland, Germany, parts of other western European countries, and North America), those who manage industry and perform its higher technical functions are native, as are also the central core of skilled workers. At least, they are native to this general area and feel themselves at home in

it. Even within these areas, however, managers and technicians may be ethnically somewhat strange to the particular smaller regions in which they work. Many of the founders, engineers and skilled workers of the early industries in the German Ruhr came from Britain and Belgium where coal mining and the iron industry had first developed machinery. It was not long, however, before Germany could supply such people in number sufficient for her own industry and for export.

There has been a constant flow of managers, accountants, engineers and skilled workers from these mother countries to the outlying newly industrialized regions of the Western world itself, as well as to the colonial regions. This movement takes them, and the industries they operate, into regions where they are ethnically strange enough for it to be remarked by themselves and by the native population. Examples are the central and eastern countries of Europe, the French parts of Canada and even our Southern states. As the prime movers of industry get out into the less industrially-minded parts of the Western world one begins to hear from them those impatient complaints about the perversity of local institutions and people which they utter more openly in colonial regions.

In the older industrial regions of the mother-countries the rank and file working force was generally built about a nucleus of people native to the region and of the same ethnic kind as management. But as industry grew, large and long-continued internal migration and immigration from other countries were necessary to keep up and expand the working force. The consequent ethnic differences are of the order of national differences within the European world. Of the mother-countries, only the United States has recruited a sizable part of its labor from outside the European cultures and races. Such people, Negroes, Orientals and Latin American mestizos, were generally brought here as labor, not for manufacturing but for industrial agriculture and construction work, just as in the colonial areas. It has been only after a long experience in the

Euro-American culture and economy, and usually after turn-over of generations that people of other than European ancestry have found their way into the labor force of American manufacturing industry. In spite of these facts, the ethnic differences show up in the strata of power, skill and prestige in industrial organizations in all Western countries. To the extent that they do not, it seems to be because the ethnic differences have, in effect, ceased to exist.

Now these mother-countries of modern industry all have open social-class systems and social ladders of a similar kind. Social mobility is part of their philosophy. The Western communities in which industry is established generally have landed, commercial and professional middle-classes who, being accustomed to prestige and power, may be jealous of the new leaders of industry. This jealousy is perhaps more apparent when the new leaders of industry differ ethnically from the local middle class. On the other hand, the latter often encourage the coming of industry, and seek to take advantage of it by speculating in land for industrial sites, and by building new houses or businesses which, they hope, will be patronized by the incoming industrial population. In any case, some combination of cooperation and antagonism of local middle-class people with the leaders of industry develops; this becomes a major theme of politics. To it is added the politics of labor, as the industrial working force takes form and becomes defined in its own eyes and those of management and the local non-industrial middle class. Some of the workers, whether native or immigrant, will try to rise in the industrial structure; their success or failure will almost certainly become symbolically important to the workers at large. If it be failure, the flames of ethnic consciousness will be fanned thereby, and local politics will reflect the fire. There may develop a labor movement which defines its enemy, management, in racial or ethnic terms, and which may, at the same time, endeavor to keep out workers of other ethnic or racial kinds. Since these are areas in which all, or nearly all, classes of people are accustomed to take some part

in politics, and therefore may be quickly mobilized against industry, management cannot ignore local politics. The various political alignments which develop in such industrial communities, and the circumstances which change them have been talked of, and have, in a few cases, been described and analyzed.³ There is need of systematic comparison of them in a variety of situations.

Although it must reckon with unfavorable alignments and opposition, industry can count on finding in the Western world the basic matrix of law, institutions and ideologies in which the special institutions of capitalistic industry grew up. I think it a fair generalization that in the mother-countries, industry prefers as little interference of government as possible, excepting only use of the police power to protect their "property" and "right to operate," and of legislation to protect markets. In such countries, industry is, as a rule, against attempts to restrict the flow of labor into and out of the region. An exception appears in marginal areas which share some of the characteristics of the colonial world; such as in the southern United States, where an industrial agriculture had developed slavery and when that was abolished, other devices for holding people to their jobs—a pattern taken over to some extent by early manufacturing industries in the region.

At the opposite pole from the Western mother-countries of modern industry, stand the colonial areas of the world,⁴ whither men of European extraction have gone or sent their agents to gather and fetch to world markets vegetable and mineral products wanted for consumption or manufacture in the industrial mother-countries. The first capitalistic industrial enterprises in such re-

gions are usually plantations or mines, which marshal large numbers of native people to labor under what is to them an alien system.

Often not enough native people can be immediately recruited to meet the newcomers' demand; in this case, laborers may be imported under indenture or contract from some other non-industrial, non-European country; generally, the imported labor has come from among the non-tribal peoples of Asia where large masses of landless people accustomed to wage-work are willing to hire themselves out for a period of years without hope of advancement. When their terms of work are up, they often go into small trading or commercial farming. They thus become a middle caste of small entrepreneurs, as have the Chinese in the East Indies and the East Indians in South Africa.

The natives themselves are often not accustomed to individual wage work, at least as a continued and sole means of getting a living. They may have worked only as members of communities, their tasks and their rewards determined by their places in a social system. Hence they are not always willing to work for long periods, if at all, in the new enterprises. In such case the political force of the colonial power is used to recruit and hold workers. A head-tax payable in money of the new system may be used to compel families, tribes and communities to send out members to work for a time. Penal sanctions may be applied to those who leave jobs. Restrictions on movement in and out of the district and earlier, colonial chattel slavery served the same end.⁵ In short, the usual economic incentives do not bring in a labor supply. Industry departs from its mother-country practice of encouraging free movement of labor, and uses the police power instead. The problem of early industry in

³ e.g., W. L. Warner, and J. O. Low, *The Social System of the Modern Factory*, New Haven: Yale University Press, 1947.

⁴ For a general definition of colonial status see Raymond Kennedy, "the Colonial Crisis and the Future," in R. Linton, *The Science of Man in the World Crisis* (New York, 1945.) He notes that Japan is (or was) the one non-European nation to have developed modern political and economic colonies, and that these colonies showed the essential features of colonies of European countries.

⁵ For material on this subject see: J. H. Boeke, *The Structure of the Netherlands Indian Economy*, New York: Institute of Pacific Relations, 1942. John A. Noon, *Labor Problems of Africa*, Philadelphia: University of Pennsylvania Press, 1944. Sheila Van der Horst, *Native Labour in South Africa*, London: Oxford University Press, 1942. Sydney Olivier, *White Capital and Coloured Labour* (London, 1906), and *The Anatomy of African Misery* (London, 1927).

England was to make the people free to move from parish to parish so that they might be available to industry; one could assume that the poor would use the freedom in a way profitable to industry. In colonial regions, that assumption proves not true, and there is a complete reversal of tactic.

This raises the whole question of the relation of industry to the law, institutions and mores of the communities and regions where it establishes enterprises. In the Western world the representatives of industry claim to believe in as little government as possible, and generally claim to respect the mores, religion and social beliefs of the communities in which they operate. Indeed, local custom and belief are often plead by industrial management as their reason for racial and ethnic discrimination and segregation. On the other hand, industry has eaten away at many customs and beliefs by its very insistence on continuous operation. But, in the main, the law, sentiments and symbols of the community are essentially those of the leaders of industry themselves. In the purely colonial regions, they are not. Local institutions and law may not allow for the kind of organization that industry regards essential to its operation; and the policing power may be neither strong enough nor properly minded to support industry. The evidence from colonial situations makes it appear that where the local legal and institutional framework stands in the way, industry is prepared to modify it as much as need be. This it does by support of imperial interference with local authority. An additional means is the establishment of separate industrial communities in which industry and its representatives exercise political and police functions over their employees: thus the familiar colonial institutions of the labor compound, the plantation, and that institution of areas marginal to the colonial world, the company town. One of our tasks should be the close analysis of the behavior of industrial management toward local law, institutions and beliefs in a whole series of situations, from those in which local society is apparently most favorable to industry to those in which it is least favorable. At the colonial pole we

get evidence of a belief in the divine right of industry to modify any society as much as need be to allow it to exploit local markets, resources and labor; perhaps it is undertaken with the least pang when the local people and culture are of some kind with whom industrial managers feel little human identification. This is one meaning of the "white man's burden."

On the other hand, it may turn out that industry will lose its sense of identification with the law, institutions and people of an older industrial community if they develop in a way unfavorable to industry beyond some point of tolerance. It is possible that law, institutions and sentiments are most favorable to industry in Western communities with well-developed concepts of the law relating to property, organization of voluntary corporations, free individual contract, and the like, but not yet highly nor long industrialized—communities still virgin, but ripe for willing embrace.

To resume our comparison, as a result of the importation of labor from other regions, the industrial hierarchy in colonial areas often consists of several ethnic groups, each of which performs some rather distinct function.⁶ The new recruits of each group come into the structure at a given level and tend to remain there. There is no ladder of promotion by small rungs from the bottom to the top of the structure. Mobility tends to occur mainly by leaving the industrial organization for some new commercial or service function brought into being by the social revolution accompanying the growth of the new economic system. There is almost complete absence of the kind of industrial mobility which is so strong in the ideology of industry in the Western countries.

A related feature of the colonial regions, well described by J. H. Boeke⁷ in his works on the Dutch East Indies, is that the native labor is for a long time only halfway in the new system. When not working in it for

⁶ R. D. McKenzie, "Cultural and Racial Difference as Bases of Human Symbiosis," in K. Young, (Ed.) *Social Attitudes*, N.Y., 1931; Sydney Olivier, *op. cit.*; Sheila van der Horst, *op. cit.*

⁷ J. H. Boeke, *op. cit.*

wages, they are absorbed again and kept by the familial, tribal or village societies to which they still belong. In course of time, the power of the native society and economy to re-absorb the industrial workers declines, through loss of land to the new system of things or through pressure of population. At the same time, some of the natives become so weaned from their mother culture as to have no wish to return to it. Their goals are already turned toward the new life. Thus there grows up a group of people who are completely dependent upon the new industrial system, people who—when not working for wages—must now be considered the unemployed of the new system. Such people are inclined to become discontent, to demand a new and higher scale of wages so that they can buy the consumers' goods of the new system, and even to demand that they be allowed to climb higher in the industrial hierarchy.⁸

A similar cycle occurs even in the mother-countries of industry when people of "backward" rural regional or ethnic elements are drawn into the less skilled jobs of industry in times of acute labor shortage. We have seen it in the United States in the case of the rural Southern people, both Negro and white; and in Canada, in the case of "backwoods" people from Quebec and the Maritime provinces. It is a process which contains the problem of hidden social subsidies

to industry, and the question whether industry can maintain the level of profit which it has come to expect when all such hidden subsidy has wasted away and the population must be kept from industrial income even when not working for industry.

In the colonial regions, there is either no middle class of natives in the European sense (South Africa), or the middle class (as in India or China) is far removed from any place in industry or control over it. The middle classes may pick up some crumbs of prosperity or power from the presence of the new economic order, or they may be threatened and destroyed by it. In either case, they may be without political power. The masses are at first politically inactive, but may begin to show signs of forming new groups, of a feeling of nationality where once there had been only tribal or village solidarity. A new nationalism may arise, and economic and ethnic unrest may be joined in it. Hence that confusion of racial and class conflict so common in the colonial areas of the world.

Within industry itself, one finds in these colonial areas almost no admission of the native population to the inner and higher positions of prestige and control. The tendency to exclusion of ethnically alien elements is here seen in its extreme; or perhaps it is only more visible because of race and the sharp cultural distinctions between the working many and the managing few. Where this line of effective exclusion is drawn depends upon circumstances and upon the nature of the industry. In South African mining, white men came from England to do the skilled work at first; now that a native labor force which could do the work has been developed, the politically active white men use the full power of the state and of racial solidarity to preserve their own monopoly. The line is held at the gate to skilled work; industry puts up with it at great cost. In other cases the line is drawn at supervision and authority. In others, the main concern is to keep merely the higher control of policy and money in the hands of representatives of the dominant, European group.

⁸ For one of the best analyses of this process see A. W. Lind, *Economic Succession and Racial Invasion in Hawaii*, University of Chicago Libraries, 1936. In this work (p. 404) is the following passage: "The process by which Hawaii imports large numbers of unskilled laborers from various sections of the globe, exploits their labor power for a few years on the sugar and pineapple plantations, and at the same time initiates them into the great American scramble for a place at the top of the economic ladder, is apparently irreversible and is cumulative in its exactions upon the existing economy and culture. Each new generation of plantation workers occasions an addition to the surplus of competitors for the preferred positions within the system, not alone by the graduation of the majority of its number to the ranks of non-plantation pursuits, but even more by the creation of a second generation even more thoroughly inoculated with the American success virus." See also his, *An Island Community: Ecological Succession in Hawaii*, Chicago, 1938.

Here we meet that peculiar phenomenon, the straw-boss, and can see his essential function. A native is given supervision over native workers; or a person of some ethnic group alien both to management and the mass of workers is given this function. The notion is that such a person will know the peculiar ways of the workers, and will deal with them accordingly. He is a liaison man, a go-between. And wherever there are workers of some kind extremely alien to industry and to the managers of industry, someone is given this function. He documents, in effect, the gap between the higher positions and the lower; and symbolizes the fact that there is no easy ladder of mobility from the lower position to the higher. He may be literally bilingual, transmitting the orders given in the European tongue into some vernacular; he is also bilingual in a broader figurative sense. He understands the language—the symbols and meanings—of the industrial world, and translates it into symbols which have meaning to people from another culture, who live in a different set of life-chances. And here we can begin to push harder toward comparison of the mother-countries of industry and the colonial industrial regions. In the latter, the straw-boss symbolizes limited mobility. He is himself mobile, and ambitious. But the nature of his job rests on the lack of mobility of the masses. In the mother-countries, the straw-boss turns up, too. He is found wherever some new and strange element is introduced into the labor force in number. The Negro personnel man is one of the latest straw-bosses; he acts as a liaison man between management and Negro help. He cannot himself be considered a candidate for any higher position or for any line position in industry; his is a staff position which exists only so long as Negroes are hired in fairly large numbers, and so long as Negro help is considered sufficiently different from other help to require special liaison. If the race line disappeared, or tended to disappear in industry, there would be no need of the Negro personnel man. There might be personnel men who are Negroes. Thus, the Negro personnel man is performing a racial

function; he is not part of the regular line of authority, and does not represent a rung in the ladder of regular advancement to higher positions. Industrial organizations in the colonial regions abound in such liaison positions. Just what such positions are, the features of social and industrial organization which they reflect, and the kinds of persons who fill them are all matters whose further analysis would throw light on the nature and internal functioning of industrial organization. It is but one of the several features which appear in clear form in colonial industry, but which may also exist, although commonly overlooked, in the industry of the mother-countries.

Now these considerations of structure and mobility again raise questions concerning the fundamental ideology of industry. For industry in the Western world promoted an ideology of mobility, that is, of ambition. In the colonial world, ambition is often regarded as unjustified and dangerous. Even in the Western world, managers speak with nostalgia of the unambitious first-generation of Poles, French-Canadians or peasant-workers of other ethnic groups; people who were content with their jobs, willing to work hard without hope of advancement. Of course, such people often had objectives outside industry to keep them at work and content; notably, the desire to save money for buying property. In spirit, they were not completely industrialized. A second or later generation which insists on advancement within industry is compared unfavorably with their fathers. The hostile reaction of many managers to ambitious Negroes is too well-known to require documenting.

Here is apparently a contradiction: Industry encourages ambition, and complains a good deal about lack of it. On the other hand, it praises some people for not having it, and complains of others who do. This raises another general question: Just how much ambition does an industrial organization want, and in how many people and in what kinds of people does it want it? In the colonial world, there is generally a limit on the possibilities of promotion for persons of each ethnic category, although this may

change through time. For certain kinds of work, it may actually be to the advantage of industry to hire only people whose ambitions are directed to goals completely outside the industrial system. For others, they may want ambitious people. There may, however, be some balance between the proportion of ambitious and unambitious people which works best even in the oldest of the industrial regions. A clue appears in a phrase current in a large concern in this country. They have a breed known as the "Thank God for" people; the unambitious people who can be counted on to stay where they are, and who keep things running while others are busy climbing the mobility ladder from one job to another. Analysis might show that in the mother-countries of industry some adjustment between symbol and reality has occurred, so that a large proportion of workers may give lip-service to the mobility ideal, but not too many take it seriously.

Just what proportion of ambitious workers industrial organizations of various kinds can tolerate is a question which merits comparative analysis, although it may be difficult to make the necessary observations in a society where people generally claim to believe in ambition and to be ashamed of lack of it. In colonial regions, the talk on the subject is often franker.

I have already noted that ethnic exclusiveness tends to develop at all levels of colonial industrial hierarchies. The dominant managerial and technical functions remain pretty much in the hands of the founding ethnic group. Sometimes a European group of skilled workers, as in South African mines, holds to its level of jobs and succeeds in excluding the natives. In the less skilled jobs, some group of natives may manage to keep out others. Even in American industry, such a tendency shows clearly. A number of forces apparently play upon hiring and selection to reinforce or to break up this tendency. If I were to venture an hypothesis it would be something like this: the tendency to exclusiveness is present in all organizations, and in the segments thereof, but the power to maintain it varies. In industry, the neces-

sity of keeping a full labor force operates against exclusiveness in those categories where large numbers are required; generally, the lower levels of skill. The people at these levels have little or no formal power of hiring. They have, in varying degree, informal power of selection and rejection.

The people in the higher levels of the hierarchy have the power to keep their own ranks ethnically exclusive. In the colonial or semi-colonial industrial regions, management often quite frankly talks of the necessity of keeping management in loyal hands; that is, in the hands of people closely identified with one another by national sentiment as well as by general cultural background. In the mother-countries of industry, one does not hear such talk, but it is possible that the mechanism operates without people being aware of it. It may operate through the mechanism of sponsoring, by which promising young people are picked and encouraged in their mobility efforts by their superiors. In the course of their rise, they are not merely given a technical training, but also are initiated into the ways and sentiments of the managerial group and are judged by their internal acceptance of them. Ethnic, national and class loyalty are undoubtedly factors in the original choice of people to be sponsored and in their later rise. In the Western world individuals ethnically different from those at the top of management may be drawn into the sponsorship circle, but in course of it effectively lose all symbols of identification with the ethnic group from which they have come and take on those of the receiving group. Where skin-color and other racial features are involved, this is not so easy to do. Thus, while modern industry is opposed to nepotism, as contrary to the choice of the best people in an open market, as an operating organization it tends to hold power in the hands of a group whose new members are picked from among people thought to be loyal not merely to the particular organization but to the management class and its culture. In the selection and sponsoring process ethnic background plays a large part.

The sponsoring power of lower ranks may

be less, but is by no means completely lacking in many situations. Coal miners and railroad workers notoriously have great sponsoring power. And even in the colonial regions the members of an ethnic group or clan, or the inhabitants of a village, may have, in effect, the power to recruit new workers. In a sense, when industry brings in some new ethnic group it has to do it in opposition to the present workers. The actual ethnic composition and changes therein seem then to be a resultant of the operation of demand for new help against the exclusive tendencies of the various segments of the existing working organization. The search of modern industry for new help that can be used with profit has certainly been active and persistent. On the other hand, for a given kind or level of job, the field in which the search is made may be limited by management's own state of knowledge and sentiments. Certainly the evidence is clear that in the colonial regions, and to some extent in the mother-countries, there grows up a body of belief about the special working qualities of various ethnic groups. These stereotypes, which may or may not correspond to the facts, act to limit the vision of those who select help and who initiate sponsorship. In a sense, this is like any marketing situation, in that the bargain-

ing of the marketer is limited by his own knowledge and sentiments. The role of sentiments is, however, made somewhat stronger in the hiring and utilization of human labor than in the buying and selling of inanimate commodities by the fact that the human labor is, so to speak, consumed by industry. Industry is not a labor broker, for it uses the labor to build a continuing organization for work; it must live with its laboring people. And in the course of working together the social and political processes get under way as they do in any organization. Industry thus considers its people not merely as technical help, but as actual or potential participants in a struggle for power within industry and society, and as potential close colleagues (or as unfit to be such). When one takes these points into account, many of the contradictions and paradoxes in the behavior of industrial management and workers begin to move toward possible solution. A complete resolution of them might be approached by systematic comparison of the various situations in which industry has operated. I suspect that in such comparison racial and ethnic differences will act as a sort of litmus paper to bring out characteristics and processes which might otherwise be overlooked.

THE NATURE AND CONSEQUENCES OF BLACK PROPAGANDA*

HOWARD BECKER

University of Wisconsin

I

FOR PRESENT purposes, propaganda may be defined as the planned use of any means of communication to impose one's will, in an actual or potential conflict situation, on anyone not ordinarily disposed to acquiesce peacefully to that will. "Impose one's will" is the key phrase, and should be discussed at length, but space does not permit.

In this definition questions of truth or falsity, as determined by any standard, are not raised. Propaganda which achieves its end may be entirely true, or it may be entirely false; expedient rationality alone governs the choice of means. When *long-time* imposition of will is the end in view, it may be highly expedient to restrict propaganda to the realm of truth, but that is a matter of strategy *versus* tactics which need not be considered at this point; it will come up later.

"White" propaganda is that variety which is definitely announced by the propagandizer as coming from a source outside the propagandized. "Outside" is the key term; it may refer either to "outside" a single personality or to "outside" groups ranging in size from pair to national aggregate. Ordinarily the reference is to groups; an advertiser propagandizes a given public, a frontline "hog caller" uses a loud speaker to call on a regiment to surrender, and so on.

"Black" propaganda is that variety which is presented by the propagandizer as coming from a source inside the propagandized. "Inside" is the key term; here again the scope is the widest thinkable. A salesman may so propagandize a prospect that the salesman never appears as such; the prospect

makes his decision on the basis of information furnished him by other persons whom he unwittingly identifies as fellow-prospects. This information he then self-deludingly interprets as his own. In this sense, then, the source is presented as inside the propagandized. Like white propaganda, however, black is usually directed at groups; a subversive rumor in a battalion is started by an enemy agent in the guise of defeatist utterances made by its high-ranking officers; a representative of an old-line automobile company manages to have a booklet presumably written and financed by a disgruntled purchaser of a new make of car circulated among others contemplating purchase, and the like.

"Gray" propaganda is a misnomer occasionally encountered. The term usually rests on the erroneous introduction of the veracity criterion; as previously stated, a workable definition does not include the question of how much of the propaganda is true or how much is false. Gray is incorrectly thought of as a mixture of the two. A second erroneous use of the term sometimes met with is in the effort to label the probably low credibility of certain black propaganda. If it is held unlikely to encounter belief in it as really coming from outside, it is called gray. Although the introduction of the credibility criterion is perhaps not as damaging as the veracity criterion, experience has shown that it can lead to confusion, and should be rejected.

Black and white propaganda are all that need be considered, and this paper confines itself to black. The division between the two was recognized during the recent war; the Office of War Information (OWI) was restricted to white, and the Office of Strategic Services (OSS) to black. Over and above them both was Psychological Warfare, an armed services organization, but this exer-

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cised only the loosest supervision, and where black propaganda was concerned, issued very few if any written orders to field operatives.¹ In the very nature of the case, black had to be kept in a position where it could be honestly disavowed at any time; otherwise public officials and even some military men, objecting to the essential deceit involved in any black operation, might cripple the enterprise.²

It should also be said that our consideration of black propaganda can be limited, to a considerable extent, to the strategic type. Strictly tactical deception, such as issuing false orders during emergencies when there was no time for the enemy to check their authenticity, was largely under the direct supervision of Psychological Warfare anyway; the few OWI and OSS men working

¹ It must be clearly stated that no classified documents of any sort whatever are referred to in corroboration of the analysis and evidence presented in this paper. The writer's memory is the sole source, and at every point where existing security regulations would seem to prohibit discussion, those regulations are observed. Moreover, dates, names, places, and terminology are disguised whenever there may be legitimate doubt as to the wisdom of providing identifying details. The writer's connection with the Office of Strategic Services terminated in September, 1945. That organization itself passed out of existence a month later; consequently the paper has not been submitted to anyone for security check. Self-imposed restrictions, however, are perhaps quite as effective as any formal censorship. In any case, the reader is entitled to know just how much or how little credence can be given to the statements hereafter made.

² A self-imposed restriction of character quite different from that noted in footnote 1 is that having to do with the ethics of propaganda in general and of "black" propaganda in particular. In the space available, it is impossible to treat the ethical problem in any satisfactory way, and the writer has therefore decided to leave it out of consideration here. This decision has been a difficult one to make, for ethical issues are of course crucial where the *ends* of propaganda are concerned. So crucial are they, in the writer's estimation, that this estimate was the final basis for the decision to omit discussion of them here. To say nothing is far better than to deal with fundamentals superficially. See the writer's article, "Supreme Values and the Sociologist," *American Sociological Review*, VI (April, 1941), 155-172, for an attempt to present some aspects of the ethical imperatives of science and of other fields of human activity.

tactically in this way were usually on detached service. Further, strictly tactical deception has little of the element of "impose one's will" in it, and peaceable acquiescence is not necessarily sought. There is some doubt as to whether it should be called propaganda at all.

Strategic black propaganda has, as the adjective "strategic" implies, goals transcending the immediate combat situation. What is tactically a defeat may be strategically a victory, and *vice versa*. If an occupation lasting for an indefinite period is planned, to the end that the defeated may eventually acquire action patterns having a minimum of overtly aggressive components, then even wartime strategy becomes in a certain sense tactical. A propaganda victory during the combat period may be a propaganda defeat in the occupation period if the victory is gained at the cost of the long-run objective. Hence it is of the utmost importance, where propaganda of any variety is concerned, to have a foreign policy that does not depend on leading personalities of the moment, an armed forces hierarchy, or the whims of the electorate—but that is another matter. Our immediate concern is with black propaganda that is not governed by hour-to-hour combat tactics, and therefore is strategic in degree at least.³

II

Among the more important kinds of black propaganda is the planted bit of gossip or rumor. Such rumors of course proliferate rapidly, and often become changed almost beyond recognition as they spread. Easy to plant but hard to control, they can usually be relied on only for morale-sapping, confusing, and disorganizing purposes. They may be useful in "softening up" an opponent in preparation for wedge-driving, but the actual driving of the wedge—between Nazi

³ Almost all of the illustrative material used is drawn from the writer's radically contrasting experiences in Germany with OSS (1944-45) and with Military Government (1947-48). As *Chicken Little* said, "I saw it with my eyes, I heard it with my ears, and some of it fell on my head."

Party and German Army, let us say—requires tools of greater precision.

Another word-of-mouth kind of black propaganda is the planted witticism, ironical remark, or joke. Ranging all the way from the simple pun to the elaborate "build-up" anecdote, the joke has the advantage of remaining relatively constant while being disseminated; it does not have the inherent instability of the mere rumor. Its chief disadvantage is the fact that it may not actually "soften up" the opponent, but on the contrary may heighten his morale by releasing tensions which would otherwise find damaging outlets. When black it is certainly more effective than when white, but nevertheless the joke requires great skill and caution in its use.

Planted slogans and visual symbols also have the advantage of remaining relatively unaltered during transmission. When the word-of-mouth repetition of a subversive slogan is excessively dangerous, resort may be had to the night-time chalking or painting of it on walls, advertising kiosks, etc. Although sometimes quite risky to carry about, for obvious reasons, the sticker, printed or otherwise previously prepared, is also a good way of getting slogans and symbols before a wide public with a minimum of personnel. At times it is possible to use the opponent's slogans in a negatively subversive sense; "Every sacrifice for victory" and "For this we have the Führer to thank" made excellent decorations for bombed German public buildings. Further, the possession of stickers bearing these slogans was not *ipso facto* incriminating at first glance. Positively subversive slogans are harder to devise, for they must proceed under their own steam, as it were. Moreover, they must be fitted into the culture pattern with greater care, and are much more dangerous to the disseminator. The writer knows of no positively subversive slogan or symbol that was widely spread in Germany as a result of black operations; most of them remained local and were short-lived.

Leaflets are primarily tactical instruments, used for front-line attacks on morale, and when so used are nearly impossible to pre-

sent black. Occasionally good black leaflets of strategic importance were disseminated in Germany, chiefly among factory workers where sanitary facilities provided ready means of disposal. Much better results were secured, however, by printing black newspapers and tearing them up. Relatively little risk is involved for the distributor, and a surprising amount of reading is done under the appropriate circumstances.

Black newspapers in general, however, are very hard to produce. The greater the length of a document, the greater the chances of detection. Type faces, makeup, style of journalese, and a thousand and one details increase difficulties *à la* an exponential curve. Even the way in which news stories are constructed may reveal the source. For instance, American journalists are trained to get the gist of the news story into the first paragraph, and preferably into the first sentence. The second paragraph repeats the first in other words and with greater detail. The third and succeeding paragraphs add local color, more detail, and so on. The make-up editor can then fit the story into the page by cutting it at any one of two or more places. German journalists, however, exactly reverse this procedure; oftentimes the nub of the story comes only in the last sentence. From this it is easy to see how insufficient ability to define the situation as the subject defines it may utterly ruin an otherwise good black newspaper. Nevertheless, at least one respectable black job was produced by the British, and until forced to "go tactical" by higher authority, received a fairly good credibility rating as determined by prisoner interrogation, reports of agents, and other intelligence sources. Whether by accident or design, this paper achieved some additional popularity because its title, *News for the Troops* (*Nachrichten für die Truppe*), could easily be punned into *News for the Clap* (*Nachrichten für die Trippe*). Given the German love for puns (even Shakespeare is highly valued by Germans for this as well as for other reasons), a great deal of free and valuable publicity was secured. When well and credibly presented, the black newspaper has great strategic advantages. News

can be editorialized as thoroughly as, although let us hope more subtly than, the *Chicago Tribune* makes its standard practice. Full-length ideological articles, positive and negative, can be provided, and if native journalists having sufficient integrity, intensity of conviction, identification with the "best interests" of the group being propagandized, and persuasive skill can be secured, long-term propaganda effects may be forthcoming. Unfortunately, most journalists of this type were too rigidly supervised by higher authorities who lacked the training necessary to define the situation properly; the results were all too often pedestrian and uninspired. When aiming at mass effects, a heavy and sincere emotional content is a vital necessity; unless the black newspaper has this, its drawbacks are so great that little if any effort should be wasted on it.

Syndicated gossip columns of Winchell, Pegler, or Pearson type are virtually unknown outside the United States, France, and Great Britain, although they are now beginning to be imitated elsewhere. If the group to be propagandized does not have the gossip column in its culture, the "poison pen" letter may prove a useful substitute. It is not a mass weapon, but can be very useful in wedge-driving and similar operations, some of which carry it to the verge of the merely tactical, but which in some cases can have strategic bearings. It is most effective when it is essentially true, needing only official investigation, for example, in order to verify its innuendoes. Naturally it must be used with the greatest of caution, for the elimination of Party corruption, to take an obvious instance, may strengthen the regime rather than weaken it. Obviously the suspicion-casting letter of greatest power is not anonymous; good forgery is a requisite. In order to secure allegations that have a high likelihood of verification, the co-operation of an espionage branch is virtually essential, although occasionally a shot in the dark on the basis of hearsay will produce surprising results. All black propaganda requires operatives thoroughly acquainted with every relevant aspect of the society and culture in question, but here the poison pen

letter makes demands second only to black radio. The letter is so highly personalized that mastery of the most intricate detail is necessary if it is to be credible. Self-defeating if used too frequently, it is a little-known but powerful black tool.

Black radio, one of the most interesting of all forms of propaganda, is the kind with which the writer is most familiar. There have been several stories in the British and American popular press about it, one of them by General Donovan's son-in-law, who ran Operation Annie at Radio Luxemburg—a primarily tactical job. Another was a British account of the Soldier's Station, West; this was a mixture of strategic and tactical, with the tactical gradually winning the upper hand. Nothing has yet been printed, to the writer's knowledge, about an American sending, here called Operation Frolic, which was designed to be strategic and yet which had surprising tactical results. But more of this later.

Black radio may take two well-marked forms: one is the so-called "free ride." This is better known as "ghost voice" interruption. After the war between Germany and Russia began, the Moscow radio did a good deal of this, but from the terminology and other details it is clear that no black credibility was expected; it was intended to be white. Earlier in the war, while Russia and Germany were still bedfellows, there was considerable ghost voicing during Hitler's speeches; from explicit announcement, accent, and many other criteria the interruptions purportedly came from an underground station in Germany which was rumored to be a mobile, truck-mounted sender. Actually, the ghost voice was beamed into Germany from a point in England, but the possessor of an ordinary radio naturally has no direction-finding equipment. It sounded like any domestic German broadcast. The German Propaganda Ministry of course soon discovered where it originated, and widely proclaimed that the enemy was at work, but the rumor of the mobile truck had been so shrewdly planted, well in advance, that a great many Germans still believed that the interruption came from inside. The free ride

has great advantages, for it cannot be jammed without simultaneously jamming the speech on which it is, so to speak, parasitic. If one is heard, the other must be heard too. The drawback of the free ride is that a connected broadcast cannot be presented; its role is limited to sarcastic comment, denunciation, and other brief interludes.

The other form of black radio is the "own steam" sending. Advantages and disadvantages, as can readily be seen, are reversed. Jamming, however, is not an unmixed evil where the propagandist is concerned, for it takes nearly as much power to jam a sending as it does to make the initial broadcast. Consequently, when a station of 110 kilowatts, let us say, is broadcasting, the opponent's stations cannot operate if jamming is to be carried out; they must leave their own wave length, tune in on the wave-length to be jammed, and devote at least 80 kilowatts to filling the ether with meaningless noise. Further, short-wave sending makes jamming very difficult except in the immediate vicinity of a powerful jammer. The Heaviside layer, fluctuating temperature, and other conditions give the short waves an irregular although roughly predictable "bounce." Hence technically skilled beaming at areas remote from powerful jammers may get the black sending through in spite of all opposition, and adept simulation of background noises and so forth may make even a powerful beam sound like a weak sender—like the famous mobile truck, for example.

Take it for all in all, black radio is a very powerful means of propaganda in areas where privately hearable receiving sets are found in numbers of any significance. At the same time, it imposes by far the greatest demands of the propagandist. One slip, and the given broadcast not only loses credibility but future black sendings are almost certain to encounter an increased number of skeptical listeners. One amusing slip, which by fortunate accident did not discredit future sendings, was that occurring when the well-known Gustav Siegfried Eins program went off the air. GSE had been running for about fourteen months, and the mobile

sender yarn was beginning to wear thin. Germans found it harder and harder to believe that a roving radio truck could elude the Gestapo for so long a time. It was therefore decided to kill off the redoubtable Gustav, and with appropriate sound effects the deed was done and properly recorded for broadcasting. Unfortunately, the technicians in charge of the GSE broadcast were not briefed as to the character of the sending, and they had become somewhat bored as well. They therefore did not listen closely to what they were transmitting, in spite of the fact that the "Gestapo raid" which eliminated GSE made a lot of unusual noise. After the customary interval, they proceeded to transmit Gustav's Last Stand again; he was killed off twice! Fortunately, the Germans were jamming GSE heavily on the first broadcast; only a very small number of listeners could have heard the first killing. For the second broadcast, the technicians shifted the beam to a different area to minimize the effects of jamming, and in addition the Germans neglected to jam because they may well have thought that nobody would be likely to die twice. Consequently the second sending finished Gustav off in proper style, and a later survey (after V-E Day) showed that the high credibility rating the sending had always enjoyed had not been diminished.

As already indicated, the tactical use of black radio, often against the protests of strategic propaganda experts, prevented it from reaching full effectiveness. Further, where American operations were concerned the rivalry between black and white resulted in a partial victory for white; most of the facilities were turned over to OWI on the one hand and to Psychological Warfare as such on the other. OSS had to fit into the few nooks and crannies that were left. Only the fact that the British made the sending equipment of the Soldiers' Station available permitted OSS to run Operation Frolic, to take only one of the most successful black sendings.

III

Where the mass dissemination of black propaganda is concerned, the best results are

of course achieved by the propagandized themselves; they pass the propaganda along to others far better than agents could. This is particularly true of rumors, jokes, and chalked-up slogans, and even of leaflets and newspapers to considerable extent. In the case of radio, with its direct impact, this is not so much the case, although occasionally a listener may take the risk of inviting others to join him.

Mass dissemination of rumor and the like is furthered, naturally enough, when there is widespread distrust of ordinary news sources. Totalitarian control of information may reach the point where all newspapers handle the news in almost identical fashion. This was a problem, incidentally, that plagued Goebbels; he frequently complained that journalists did not sufficiently rewrite his propaganda handouts, so that *Der Völkische Beobachter* and *Die Kölnische Zeitung* sounded precisely the same. Even when totalitarianism is lacking, distrust of the press may arise in other ways; the recent conduct of the American electorate is here in point. Further, there may be distrust of official news sources; the fact that newspapers publish Drew Pearson and that he has hosts of uncritical readers perhaps arises from a demand for "the real lowdown" somewhat akin to the German hunger for rumor and gossip under the Nazis.

But although mass dissemination of black propaganda is best carried out by unwitting aides, there are times when witting dissemination is indispensable; this is of course true of the first stages of any black operation.

IV

Of the utmost importance in all black work is accurate intelligence and its equally accurate evaluation. White propaganda can make blunders without much damage, for the white operative has no need to keep cover. If he makes a mistake about the Führer's birthday, no great harm is done, for to most of his hearers he is an ignorant foreigner or malevolent refugee by definition—but what German inside Germany, even if irrevocably opposed to Nazism, would commit such an error? The black prop-

agandist must *know* what he is talking about; good intentions will not do.

Among the most important of all sources is ordinary overt information contained in encyclopedias, histories, textbooks, newspaper files, commercial catalogs, neutral newspapers, and the like. Frequently a special branch carries out the necessary research in these overt sources, and prepares reports for the field branches. Washington and London were full of OSS researchers of this type during the war; they were never near the front lines, but they were indispensable. Enemy overt communications are also useful, and are readily obtainable in or through most neutral countries. Evaluation of even overt enemy information must of course be carried out with great skill and circumspection; instance the fact that statistics may be faked for domestic consumption as well as for swallowing by the opponent. Further, it is usually necessary to scrutinize one's own overt intelligence with great care. All too often the black propagandist encounters a researcher's report containing a rumor which the selfsame propagandist planted three months earlier, and which now turns up in a neutral newspaper as a "hard fact" to be duly registered by the faithful research branch. It was partly because of such difficulties that black propaganda began to do its own research shortly before the war came to a close.

There is one kind of overt intelligence where collection and evaluation ordinarily go hand in hand; namely, prisoner interrogation. Given the large numbers to be handled, at most times, a rapid evaluation of the probable range and reliability of the information to be gleaned by questioning is necessary if the worthless prisoners are to be sifted out. This evaluation, under wartime conditions, is necessarily of rule-of-thumb type, but it has to be carried out in one way or another. White polling techniques were applied to prisoners in a few instances, but they are of little use to the black propagandist, valuable as they may be for other purposes.⁴

⁴ Incidentally, the same is true of white polling when used to get at the attitudes of peasants and others in liberated and/or occupied areas. In several

Official organizations collecting covert intelligence, i.e., through espionage, are manifestly indispensable. The British have their remarkably efficient Political Intelligence Division, of which the covert branch is probably most important. Other countries also have good espionage agencies; our own gained most of its recognition for work in wartime. The Alsop book entitled *Sub Rosa* gives a fairly good idea of American espionage as conducted by OSS, and seems reasonably accurate. Although OSS did have a special covert branch, it was occasionally necessary to use agents who, although primarily black propagandists, also furnished a large share of the intelligence on which the propaganda was based. In short, black operatives sometimes had to secure their own intelligence, in part because branch rivalries led those attached to espionage to be indifferent to co-operation with black propaganda. Mention has already been made of the fact that false rumors and other subversive information planted by black propaganda sometimes turned up as hard intelligence in later research reports; the same ludicrous errors appeared in espionage reports. If those evaluating covert intelligence had deigned to inspect the available records of black operations completed and in progress, they might have been able to contribute to the joint enterprise even more effectively than they did.

Covert interrogation and interception of messages furnished a substantial part of the intelligence used in black propaganda.

instances of such polling by another agency, which showed "friendly" results, OSS was in a position to check because of experience with escape routes for airmen through the French territory in question. Villages which polled "friendly" were not seldom those which were bypassed on airmen's escape maps because of high frequency of betrayal to the Germans by the inhabitants. But after all, a pragmatic check on any technique, those which became notorious in the recent election not excepted, may be of signal service to science even when the results are adverse if the technique is thereby bettered. In passing, it may be suggested that for some legitimate peacetime purposes a kind of black polling might prove to be useful in the United States, although the deception almost inevitably involved presents a difficult problem, not only of ethics but also of sheer workability. This is not the place, however, to consider matters of this kind.

Where the interrogation was concerned, most of it was done by disguised agents who mixed with prisoners, but in a few instances such agents actually worked within the German Army and among the civilian German population. Although they did no systematic polling, which would have been fantastically impractical, they were in a sense black pollsters. The greater part of the information thus secured was of tactical importance only, but it will be recalled that Otto Gisevius, who was very high in the Nazi hierarchy, was in touch with OSS, and much of the intelligence he communicated was eminently strategic. The espionage branch carried out nearly all of this subtle and dangerous kind of investigation.

When covert intelligence did not flow through official channels of one sort or another, it frequently came from volunteer enemy amateurs. Men who had failed to rise in the Party, or who were disgruntled for other reasons, or who were trying to curry favor with the prospective victors, or who were disillusioned by Party brutality and stupidity, volunteered information, sometimes at much apparent risk to themselves, in all sorts of clumsy and circuitous ways. Such intelligence is initially suspect under any circumstances, of course, and doubly so when the motivation for transmitting it cannot be imputed with a high likelihood of accuracy in diagnosis and prognosis. But although some of the persons concerned were actually Nazis posing as sacrificial idealists, and although some rapidly became professional double agents selling nearly worthless information to both sides, there were several amateurs who demonstrably contributed much, through the intelligence they volunteered, to the Nazi defeat. The writer thinks of one man, here termed Intact, who figured prominently in the *dénouement* of Operation Frolic, and who will be mentioned again later.

V

In the foregoing pages a good deal has been said about the actual utilization of intelligence, particularly where black radio is concerned. Stress was laid on the fact that a vast amount of assimilated information

about the society and culture in question must be actively in function at all times.

In addition, it must always be remembered that utilizable interpretations of intelligence must be utilizable in at least two ways. First, the black radio team, composed at a minimum of intelligence man, script writer, recording and/or transmitting director, speaker or speakers ("voices"), and operation technician must be able to interpret the intelligence so that they all agree as to what if anything they can successfully do with it. This sounds simple, but is not so by any means. For a time OSS hired Hollywood "story idea men" and Broadway advertising campaign planners "to give the dopes' dope the right slant." Although there were several honorable exceptions, they proved abysmal failures in many instances, for the intelligence items collected did not fit into any sociocultural context with which they were or could become remotely familiar. They had never learned the elementary lesson of modern anthropology and sociology; namely, that societies and cultures are intricately interwoven patterns that cannot be ignorantly torn apart and carelessly sewed together again without getting a crazy quilt for the labor. It finally proved necessary to ship a sizeable proportion of the collection of "experts" back home and let the black radio teams, made up of men who were thoroughly conversant with German and Germany, do their own interpreting of intelligence. Some of the most effective propaganda, black or white, is that which is true or could be true; the task of the team is to weave otherwise disconnected items into a coherent, plausible, and probable whole.

Second, what is coherent, plausible, and probable to the broadcasters is not necessarily so to the listeners. The chances that it will be so are increased in the degree to which the broadcasters can identify themselves with the listeners, but a certain gap always remains. Even the best actor encounters audiences that he cannot warm up, or he suffers from cold moods himself. After intelligence man and script writer have done their preliminary best, and the others have

acted in the role of potentially skeptical audience, and after the revisions their criticism entails have been made, the final draft is prepared. This must meet every conceivable requirement of verisimilitude as defined by the listeners to which it will appeal; among other things, it must ring true emotionally. Only intimate acquaintance with the society and culture, again, *plus* the elusive quality that makes good actors, enables the critics to project themselves into the roles of the listeners with any prospect of success. (The final test is of course pragmatic, although months may elapse before the validation is known to the black team. If the broadcast achieved its intended effect on its real hearers, that is that.) But before the actual broadcast, it falls to the lot of the recording director and the speaker or speakers to rehearse, record, play back, and repeat the process again and again. Sincerity or the semblance thereof is an asset even to those who do commercial broadcasting; it is absolutely indispensable to the black radio propagandist. If after repeated trials a speaker cannot carry emotional as well as intellectual conviction to his critics in their projected roles, another speaker who can do so must be found.

The mediocre radio professional usually fails; rank amateurs, whose clumsiness is an asset, or topflight professionals so slick that they do not seem slick, usually meet the test. There are of course culturally defined ways to make a radio broadcast, just as there are to write a news story. For example, the easy familiarity and jocularity of much American technique is utterly strange to the German; his *Humor* is of a different kind, and his flights of rhetoric are quite natural to him.

In broadcasting to Germany, with its well-marked dialects and its distinct dialect traces even in the speech of the highly educated, it goes without saying that the proper local twang must be used if the character of the operation demands it. Of course, there are some operations which are not particularly localized; all that is necessary is a kind of speech in keeping with the supposed status

of the person broadcasting. Here the Gustav Siegfried Eins program again provides a good illustration. GSE was cast in the role of a hardboiled German officer of lower rank who represented the opposition of the professional military men to the Party, and who stopped at nothing in retailing scandal about Nazi officials. The program was perhaps the most pornographic ever put on the air, but had the merit of being plausible and probable to the listeners, as prisoner interrogation abundantly demonstrated. Gustav's voice and manner of speaking fitted his role very well; no one, it seems, could think of him as other than a very coarse, low-ranking professional officer. His speech had Hamburg overtones, to be sure, but the rasping military staccato almost drowned out the regional accent. The man doing the speaking was a prisoner of war who hated the Nazis from his very soul; he was so bitter that he would have been unusable in most programs, but he fitted GSE to perfection. Further, his amateurishness as a broadcaster was an advantage; nobody could expect suspicious smoothness or unctuous persuasiveness from him after listening for a few moments to his hard breathing, rough intonation, and general bull-in-a-china-shop way of denouncing his despised opponents. The pornography, which was deliberately planned by the sedate British as the main listener appeal, seemed utterly natural to such a man. His interpretation of the information he imparted, most of it based on hard intelligence, carried emotional as well as intellectual conviction to his listeners.

When intelligence is interpreted so that it makes sense to the broadcasters, both in their roles as observers with superior knowledge of the relevant facts and in their projected roles as participants defining the situation as it is possible for them to know it, such intelligence has been properly utilized. Beyond this lies only the task of timing sendings correctly, evading jamming, gauging the right kilowatt strength, and so forth. This part of the job was usually done amazingly well by American radio technicians, but their skill was not always matched by the other members of their teams.

VI

So much for the nature of black propaganda; what of the consequences? Many of them have been indicated in previous pages, but not in any systematic way. Restricting the discussion to black radio, let us try to remedy the deficiency.

The credibility of any black broadcast is *finally* demonstrated only when the effort to "impose one's will" has met with success as demonstrated by appropriate action attributable to the credibility. Hence consideration of credibility should come after consideration of the correlated actions, but convenience in exposition prescribes the reverse order.

Going on the basis of what people say rather than what they do, which at best is risky at any time and not only every four years, it can be stated that there was great variation in the credibility of programs. The sources of "what people say" were prisoner interrogation, interviews with civilians by the members of the United States Strategic Bombing Survey, a survey of listeners to black broadcasts which was carried out by an OSS team supervised by the writer after Operation Frolic (for which he was chief intelligence officer) went off the air, and interviews with German radio experts.

Gustav Siegfried Eins met with high credibility (over 75 per cent of listeners initially thought that he was actually broadcasting by mobile truck from inside Germany) for about five or six months; after that, in the recollection of those who listened frequently to the program, there seemed less and less likelihood of escape from the Gestapo, and at the end of the fourteen months' run credibility was less than 10 per cent. The British would have been well advised to stage Gustav's Last Stand after eight months' broadcasting at most.

The Soldiers' Station, which was at first vaguely "located near Calais," and later even more vaguely "west," was in a sense the successor of GSE, but was inherently a more plausible operation. An isolated group of German Army men was represented as having set up a sender, and as having established intelligence connections with the home front.

Party scandal was broadcast, but the chief basis for the high credibility of the Soldiers' Station was the accurate news of home conditions it provided. Weeks before mail reached the front soldiers learned from the Soldiers' Station just what streets had been bombed out in the towns from which they came, and the news was almost always correct, for it came from Allied air reconnaissance. Not until a black newspaper with eventually low credibility, *News for the Troops*, began to duplicate in print what the Soldiers' Station said over the air did credibility begin to fall off, and it did not reach its eventual low of less than 5 per cent, as over against an early high of more than 50 per cent, until SHAEF decreed that it "go tactical." One week the Soldiers' Station was forced to broadcast in a way designed to get the German population out on the roads, with the objective of hampering the retreat of the German Army. The next week instructions were reversed, for our movements became so unexpectedly rapid that we wanted the roads clear for our own advance. The result was only what might have been expected, but the war ended in less than a month after the loss of credibility anyway. Given the heavy and excellent ideological content of the Soldiers' Station, this loss also represented a loss of strategic ideological capital which would have been useful in the occupation period. Strategic propaganda should rarely if ever be subordinated to tactical considerations; whenever possible, the line between the two should be firmly fixed.

Operation Annie, already mentioned, makes this even more clear. The apparent desire to get immediate results led to greater and greater stress on tactics. Finally, the use of the powerful Luxemburg station to divert a few German Army trucks to a route leading behind American lines and hence to capture showed how a 16-inch gun may be used to shoot a mouse.

Operation Frolic, on the other hand, never left the strategic level in any significant measure. It had surprisingly successful albeit largely unforeseen tactical consequences, but long-term strategy was the aim throughout. The plan of the operation, which

was closely followed, ran along these lines:

After two or three months' preliminary work, a program was broadcast ten to twelve times daily from about the middle of February until slightly after the middle of April, 1945. Only one script writer and one speaker were used throughout. The plan called for this in view of the fact that German defeat seemed sure to come about in not more than four months, and it was felt that the two key figures could hold the pace that long. All the skill of the American team was directed toward the task of building up a personality representing the best in the German liberal tradition, broadly interpreted. This personality, here termed Holly, was to have high ideological consistency and integrity, and in addition was to have the capacities of a man of action who could also elicit action from others at the right time.

Covert intelligence coming from Switzerland and elsewhere established two reasonably firm bodies of fact. One was that there existed a potentially revolutionary group somewhere in Bavaria, probably Munich, small in number but highly disciplined, composed partly of civilians and partly of a specialized Army unit. This group was not identified with the abortive student outburst of 1943 nor with the July 20, 1944, attempt on Hitler's life. The intelligence was provided primarily by one man, an amateur German volunteer, who took great risks in journeying to Switzerland for this purpose. This man, previously mentioned under the name Intact, had demonstrably suffered greatly although indirectly because of the Stalingrad debacle, and his type of motivation seemed imputable with sufficient accuracy to make it safe to regard him as trustworthy. He was frightened away, however, by a premature intelligence leak that might have led to his discovery by the Nazis; we had to rest content with less intelligence than would ideally have been desirable. The second reasonably firm body of fact was that Hitler planned to withdraw for a last siege into the so-called Core Redoubt or Alpine Fortress lying in the mountains of Bavaria and Austria, and roughly centering about a point a few miles south of Berchtesgaden. Beginning with

lines: preliminary to twelve of February of April, speaker called for an defeat more than the two at long. was di p a per the Ger interpreted. was to and in the ca could also time. Switzerland reason at there group Munich, d, com of a spe ot iden burst of mpt on provided German journey. This the name atly al Lingrad seemed to make. He was mature to his st con ideally onably planned -called in the , and miles with

July, 1944, intelligence concerning the Alpine Fortress was viewed with skepticism as a German plant designed to make a negotiated peace possible, but in February, 1945, we decided to stake the success of Operation Frolic on what we believed to be indisputable proof that the region had been fitted out to resist a long siege and that Hitler did plan to withdraw to it. (Investigation after V-E Day showed that we were right.) In February we of course knew nothing definite about the atomic bomb, and we regarded it as quite possible for a body of crack troops to hold out in a well-equipped mountain region long enough to result in a great number of Allied casualties and to create a new Barbarossa myth. *Ergo*, the personality of Holly, the potential rebels of the Munich area, and the prevention of withdrawal to the Core Redoubt had to be linked together in Operation Frolic.

The broadcasts began with exalted appeals to anti-Nazi Germans to revive the liberal and humanitarian traditions of the early nineteenth century and, by translating them into action at the right time, to prevent further sacrifice of the inhabitants of *all* the warring countries in a senseless final siege. Heavy stress was laid on "at the right time"; we repeatedly said, in effect, "Do not act until the Allied armies are close enough to join hands with you; do not attempt a Warsaw or a July 20th!" After this initial idealistic appeal, in which, be it said, most members of the broadcasting team themselves passionately believed, emphasis shifted to the hopelessness of German resistance and to ways and means of preventing withdrawal to the Alpine Fortress. After two months of Operation Frolic, we then went off the air with the announcement that our underground station was about to be seized by advancing American forces, and that the time for action had come. Prisoner interrogation led us to believe that the credibility of the sending was high, but action is the final test.

To repeat: we never swerved from our strategic plan; tactical considerations colored but did not dictate the content of the daily broadcasts. The idealistic note, al-

though not as strong as at the beginning, was sounded time and again, for we were thinking of the occupation period as well as of the coming military victory.

Postponing further discussion of Operation Frolic for the moment, let us now review some of the actions that resulted from black radio operations of tactical type.

The Soldiers' Sender counseled sabotage, among other things, and even gave detailed instructions on how to make explosives and incendiaries, and how to use incendiary packets dropped by Allied planes. As nearly as can be determined, little sabotage by Germans ever resulted; a few foreign workers made sporadic attempts, but nothing much came of them. The Soldiers' Sender broke its rule, in its sabotage broadcasts, of adhering to what was probably true; it carried "news" of extensive sabotage that never took place. This has been unfortunately reflected in the widespread German belief that part of the defeat was the result of sabotage; Carl Zuckmayer's recent play, *The Devil's General*, goes so far as to attribute the defeat of the German air force to sabotage by German engineers and workers beginning as early as 1941! Tactical use of black radio has already had undesirable long-run strategic consequences if out of this sabotage myth a fresh stab-in-the-back legend should develop.

The Luxembourg station, headquarters of Operation Annie, also counseled sabotage, and in addition told how to organize smaller towns and villages so that they could not be defended by the retreating German Army. To lend plausibility to the broadcasts, here termed Upset 16, a considerable amount of ostensible code, which actually was pure gibberish, was mixed with the detailed instructions. The survey conducted by the writer revealed one crackpot insurance salesman who really did organize five villages so successfully that the German Army could not use them in rearguard action. When interviewed he repeatedly asserted that his success was attributable to the fact that he could understand the code. Some casualties were doubtless avoided by this small-scale tactical success, and to this extent Upset 16

was worth while. It did a little good, and no demonstrable harm.

It would be possible to describe other black broadcasts of tactical character, but space does not permit. Let us return to the action connected with Operation Frolic as an example of the strategic use of black radio, and then to the unforeseen aftermath.

The action may as well be called by its right name, the Gerngross Aktion, for a few American news stories about it appeared shortly after the surrender of Munich on May 2, 1945. The official Army histories, as the writer has heard, make little mention of it, in part because the necessary information seems to have been screened out before reaching the hands of the official historians. The name Gerngross is taken from the German captain of the interpreter's company who organized the revolt, a man of vigor, intelligence, and integrity.

Here, then, is "the potentially revolutionary group somewhere in Bavaria, probably Munich," about which Intact had volunteered his fragments of intelligence. What connection, if any, did Operation Frolic have with the Gerngross Aktion?

In answering this question there is an express effort to refrain from unduly positive statement, for a number of reasons. Chief among them is the fear of minimizing the initiative taken by Gerngross himself; also important is the fear of over-estimating the significance of an operation with which the writer was so closely identified.

Operation Frolic went off the air with a call for action, on the evening of Hitler's birthday, April 20. This was a time when every German who could get access to a radio was listening, for it was expected that some startling last-minute proclamation by the Führer would be forthcoming. This was merely a fortunate coincidence; what primarily determined our decision to be "seized by advancing American forces" at this time was the fact that these forces were rapidly coming within striking distance of the Munich area, where we hoped that our potential rebels were located. And they were there.

In the early morning small hours of April 28, one week after our final summons, the

company captained by Gerngross seized the chief Nazi newspaper headquarters in Bavaria, the building of *Münchener Neueste Nachrichten*, barricaded a part of their number inside, and with part of the remainder seized the Munich radio station. The newspaper plant was held for six hours, and the radio station for four. The governor of Bavaria, Ritter von Epp, was also seized and placed before the microphone with a gun at his head to broadcast a general surrender proclamation. This was a mistake, for the governor refused to do so, and the microphone picked up his refusal. If he had been shot at once for the sake of the sound effects the situation might have been saved, but this was not done and the Nazis regained courage. The Gerngross forces were compelled to yield to superior numbers, but in their last-ditch stand many were killed. Major Carraciola, an officer who although not part of the company made common cause with it, was captured and executed along with several others, and Gauleiter Paul Giesler regained control. Gerngross and other leaders who escaped went into hiding. However, some members of the company who had remained outside the newspaper and radio buildings had in the interval thoroughly disorganized the Peoples' Levy, the *Volkssturm*, so that the 4Fs, old men, and boys making it up threw their weapons in the Isar River and scuttled back to their homes, or to what remained of them. Moreover, a broadcast announcing the revolt, which was called The Freedom Movement for Bavarian Reconstruction, was made before the abortive attempt to get the governor to call for surrender. This touched off the powder train of the rebellion that Gerngross had laid outside of Munich; trains were derailed, wires cut, and bridges blocked or blown up. It became clear to the Nazis and to the German Army that Munich could not be held, and on April 29 and 30 a general withdrawal began. The American forces had been poised outside of Munich from the 26th on, but did not attempt a final assault because Gerngross had succeeded, on the night of the 27th, in getting a captured American officer back through the lines with an appeal for delay. He pointed out that a bombing

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attack, which we were ready to make on a 500-plane scale, would prevent the use of Munich as a base for further American advances, and the attack was withheld. On May 2 the city surrendered to General Devers without the firing of a shot. The city where the first glorious Nazi *Putsch* occurred, in 1922, had ingloriously capitulated.

The collapse of Munich resistance meant that the American south flank was covered, and that the last dash to Czechoslovakia could proceed unchecked. On May 3 and 4, the only remaining corridor connecting the rest of Germany with the Alpine Fortress was sealed off, and withdrawal for the final grandiose siege was rendered impossible. Does this account for Hitler's last-minute decision to stay in Berlin for the *Twilight of the Gods*? Who knows?

As might be supposed, those who had been associated with Operation Frolic were somewhat curious as to what part their work had played in these thunderous events. The writer had been sent, shortly before Operation Frolic went off the air, as chief of a survey team to check the credibility of black sendings, and Munich seemed to be a good place to do a little surveying. We got there shortly after Gerngross had been placed under American house arrest. A series of interviews with him and his aides took place; these have been reported from memory in condensed form as part of the epilogue of the writer's book, *German Youth: Bond or Free* (New York: Oxford University Press, 1947). Munich is called Nirgends, Gerngross is Nimmer, the river Isar is the Schlauch, and so on.

Nothing was said in that book, however, about Operation Frolic, for the various revealing accounts of black radio which have since been published by others had not yet appeared. The interviews actually centered around Operation Frolic, but because we could not let Gerngross know, at that time, that he had been listening to a black program, we had to beat around the bush for a long while in order to elicit the desired information. At last he asked us directly, "Do you know who Holly is? He has been making marvelous underground broadcasts for the

past two or three months. *There is the ideal leader for a new and peaceful Germany!*" Naturally we could not tell him that Holly was "just five of us fellows," so we replied in the negative, but promised to look for him. Gerngross went on to say that Holly's advice to defer action until success was reasonably certain had been taken to heart, but that his suggestions about ways and means of preventing withdrawal to the Alpine Fortress were seldom if ever worth trying to put into practice. The Gerngross group had finally come to the conclusion that although Holly's ideology and his major strategy were in the highest degree acceptable, he knew too little about the peculiarities of the Munich situation to be a trustworthy tactical guide. The most that could be done was to prepare for the surrender of the city and for the disorganization of resistance in the environs of Munich and in a few key spots nearby. When Holly went off the air with a call for "Action now!", however, it seemed to Gerngross that he was probably a little closer to the realities and that his judgment was worth considering.

Let it be repeated that the writer is keenly aware of the danger of over-estimating the importance of Operation Frolic. The story has been told with as much restraint as can readily be mustered by a participant in it, and that is all that can well be said. It may be suggested, however, that here is a field for historical investigation by scholars of the highest competence; if even a few of the inferences the writer is privately inclined to draw are correct, the history of the events leading up to V-E Day needs rewriting.⁵

But this is not an audience composed of historians; we want comments of greater social-psychological relevance. It is clear

⁵ Certainly the official Army accounts might receive a little attention at certain crucial points. Gerngross is alive and well in Munich, the members of the Operation Frolic team are now safely back in the United States, and the numerous minor figures in the Freedom Movement for Bavarian Reconstruction can easily be reached for questioning. The writer himself possesses several important documents, collected in Germany since his period of OSS service, which he would be glad to put at the disposal of any qualified and interested researcher.

that Operation Frolic achieved high credibility, and that the associated action was not in any way hampered, to say the very least, by the personality and strategy of Holly. Equally clear is the fact that greater stress on tactical details might well have destroyed the credibility achieved; whatever tactical success resulted was an accidental by-product of the strategy. Black radio, and perhaps black propaganda in general, should keep well over toward the strategic end of the strategy-tactics continuum. Is that a valid conclusion?

VII

But the end is not yet. Comes the melancholy task of pointing out that "a propaganda victory during the combat period may be a propaganda defeat during the occupation period if the victory is gained at the cost of the long-run objective." What has been the aftermath of Operation Frolic and the Bavarian occupation to which it may have contributed?

The reader will perhaps recall that Gerngross was found by the survey team under American house arrest. An adverse report turned in by paid informers and signed by two bright-and-early Counter-Intelligence Corps men who understood little German but who were determined "to put all Krauts in the same pot" ran to the effect that Gerngross and his associates were merely Nazis who were trying to save their hides at the last minute. We did our best to get the report withdrawn, or at least corrected, but as Gertrude Stein might have said, a report is a report. After two trips to Munich, we reluctantly left without having remedied the injustice. We wrote a report that contributed toward the removal of a particularly stupid colonel, but that did not better matters for Gerngross in any way. We were in what came to be Patton's bailiwick, and Patton had no love for roving OSS operatives with SHAEF passes.

Nearly three years later—in March, 1948, to be exact—the writer visited Gerngross a second time. Bitterness had already appeared in the 1945 interview, as a passage (pp. 260-265) in the book referred to above

shows. In 1948 Gerngross provided a cordial reception, and then said, "Do you know, you are the first American I have welcomed in my home since you were last here? I want nothing to do with Americans beyond the necessities of routine. House arrest would have ruined my reputation had I not had a few friends who believed in me. My mail was perpetually censored, the censorship delays almost destroyed my legal practice, and I have been repeatedly denounced by cheap informers who know that the adverse CIC report is still in the files. Moreover, I now know that there never was a Holly, and that he would never have been tolerated by the occupation authorities in Bavaria if he had existed. You do not want sincere, spiritually independent, and genuinely democratic Germans; you want yes-men, clerical toadies, and apes of what you call democracy, but from what I can see is only a Coca-Cola culture. I liked you as a person, and still like you, but can you tell me what consistent policies the government you represent is now following?"

The writer did not attempt to answer, for to quote William James, "What has concluded that we should conclude with regard to it?"

VIII

Putting the matter less cryptically, it is a little risky for the sociologist, to name no other social scientist, to pronounce on the consistency or inconsistency of policies when he does not possess all the evidence.⁶ In 1945 the writer had just enough access to high-level classified documents to know that very few persons indeed knew what was really going on at any given time. (Sometimes he arrived at the pessimistic conclusion that nobody did.) In 1948 his position was of such different character that he had even less reliable information about most major policy considerations; outside the fields of education and youth affairs he knew only a little more than he learned through ordinary governmental publications and the

⁶ See L. L. Bernard, ed., *Fields and Methods of Sociology* (New York: Long and Smith, 1934), pp. 32-33.

newspapers, and that was often precious little.⁷ As Stevenson so aptly said, "The obscurest epoch is today."

Over and above this, it is perhaps permissible to point out that the sociologist as sociologist does not indulge in prophecy. The prophesying of later phases in a succession of unique events can never be scientific in any strict sense.⁸ Prediction, as contrasted with prophecy, can maintain high scientific respectability, but prediction never deals with the unique as such.⁹ Here, however, emerge far-reaching theoretical issues that are not directly relevant at this point.

One relevant issue frequently appearing in what has gone before, however, is of basic significance for contemporary anthropological and sociological theory; namely, that social life must be interpreted in con-

text.¹⁰ It is an issue because many of us who render lip-service to "definition of the situation" or "dynamic assessment" or "humanistic coefficient" do not really seek for ways and means of pragmatically validating our conclusions. Instead, we often rely on analogies, usually quite inconsistent with our proclaimed assumptions, of topological, physical, or biological character, and wind up with nothing more than the internal consistency of a postulational system as "proof."¹¹ We have successfully interpreted a given type of personality, conduct, or social structure only when we have devised crucial tests enabling us to say, "If and when these circumstances recur, these are the probable consequences."¹² Interpretation in context necessarily involves pragmatic validation.¹³

¹⁰ Becker and Hill, eds., *Family, Marriage and Parenthood* (Boston: Heath, 1948), chapter I, "Interpreting Family Life in Context."

¹¹ Howard Becker, "Limitations of Sociological Positivism," *Journal of Social Philosophy*, VI, 4 (July, 1941), pp. 362-70.

¹² H. P. Fairchild, ed., *Dictionary of Sociology* (New York: Philosophical Library, 1944), "Topology, Constructive," pp. 325-26; "Prediction, Sociological," pp. 228-29, and especially "Culture Case Study," p. 82.

¹³ It should be noted that the concluding section of the present article is in no sense an adequate theoretical evaluation of the evidence offered in the preceding sections. The numerous footnotes are merely intended to give some idea of the directions such evaluation might take if space permitted.

⁷ This was not merely because many documents meriting a classification no higher than "restricted" were labeled "top secret" by zealous bureaucrats, but also because of the very nature of the situation. See Barnes, Becker, and Becker, eds., *Contemporary Social Theory* (New York: Appleton-Century, 1940), p. 540.

⁸ Gurvitch and Moore, eds., *Twentieth Century Sociology* (New York: Philosophical Library, 1946), pp. 83-95; Howard Becker, *German Youth: Bond or Free* (New York: Oxford, 1947), p. 201.

⁹ Barnes, Becker, and Becker, eds., *op. cit.*, pp. 21-28. See also Howard Becker, *Systematic Sociology on the Basis of the Beziehungslehre and Gebildelehre of Leopold von Wiese* (New York: Wiley, 1932), p. 22.

Migration and Distance*

DONALD J. BOGUE AND WARREN S. THOMPSON

Scripps Foundation for Research in Population Problems, Miami University

THE DISTANCE which must be spanned in migrating is generally regarded as one of the principal factors influencing the number and characteristics of migrants which arrive at or leave a given point.¹ Because only fragmentary data have been available until recently, it has been almost impossible to form even a vague notion of the extent to which distance acts as a barrier to internal migration in the United States. Also relatively little is known concerning distance as a selector of migrants. Since distance is a continuous variable, in classifying migratory journeys one would like to escape the necessity of using such qualitative categories as "long" and "short" and to be able to study the effect which varying amounts of distance have upon the number and characteristics of migrants. Although data which would permit a precise and detailed distance analysis are still not available for the United States, it is now possible to arrive at a few broad conclusions, using the enumeration of migrants taken as a part of the Sixteenth Census. This can be accomplished by classifying each state by its distance relationship to every other state or division, and grouping the states into very broad distance zones with respect to every other state. While the distance "continuum" which is thus constructed is a very crude one, it is nevertheless capable of grouping migrants into distance classes with sufficient consistency to permit some basic generalizations to be made concerning this subject about which there

are few verified conclusions. This paper describes a set of distance categories which were devised on this principle, and will enumerate a few findings about distance and migration which have been arrived at through a study of the 1935-40 migration data in terms of these categories.

The Bureau of the Census has already recognized three distance categories for migration: "migration within a state," "migration between contiguous states," and "migration between noncontiguous states." The "within-state" migrants are those who crossed a county boundary or the municipal boundary of a city of 100,000 inhabitants or more in effecting a different residence on April 1, 1940 from that they had on April 1, 1935, but who resided in the same state on both dates. The distances which this group of migrants travel can vary from less than one mile to several hundred miles, depending upon the size of the state within which they move. "Migration between contiguous states" involves changing residence across a state boundary. Here, too, the distance traveled can vary from less than one mile to several hundred miles, depending upon the size of the states. However, the distances traveled by "within-state" migrants are much shorter, on the average, than those traveled by migrants between contiguous states. This difference is due partially to the fact that the maximum distance which within-state migrants can travel is smaller than the maximum distance which migrants between contiguous states can travel. To a very great extent, however, the difference is due to the fact that county boundaries are much closer to most residences than state boundaries, and hence within-state migration includes a far higher proportion of very short moves across one county boundary than is the case for migrants between contiguous

* Paper read before the annual meeting of the American Sociological Society held in Chicago, December 28-30, 1948.

¹ cf. Dorothy S. Thomas' statement that community structure, distance, and phase of the business cycle are fundamental factors in the study of migration differentials. *Research Memorandum on Migration Differentials* (New York: Social Science Research Council, 1938), p. 6-7.

states. In attempting to establish a continuum of distance traveled, there is little which one can do to convert the "within-state" and "between contiguous states" migration categories into mileage values.² These two categories must be accepted as they are reported, remembering that "within-state" migration represents a high proportion of very short moves and that "between contiguous states" migration includes a fairly large proportion of this same type of very short moves with a preponderance of journeys involving distances of 75-400 miles.

It is possible, however, to break up the "between noncontiguous states" category into a few broad distance intervals, and to classify each state according to its approximate mileage relationship with respect to every other noncontiguous state. This procedure has been followed here, the "noncontiguous states" category being divided into four distance zones.³ This subdivision of the noncontiguous states, together with the two categories which have already been discussed, permits a classification of the dis-

tances which migrants travel in terms of the following six distance intervals:

<i>Distance zone</i>	<i>Distance equivalent</i>
0	Migration within a state
1	Migration between contiguous states
2	Migration between noncontiguous states less than 600 miles apart
3	Migration between states 600-1,200 miles apart
4	Migration between states 1,200-2,000 miles apart
5	Migration between states 2,000 or more miles apart

The studies which we have made using these distance categories have been conducted either in terms of *rates* of migration or percentage distribution of migrants, rather than in terms of *numbers* of migrants. Rates eliminate those differences between zones which are due only to differences in the amounts of population available for migration. For comparing urban and rural differences, the differences in color and sex, and other characteristics, the use of rates eliminates differences in population composition.⁴

² One can assign an average distance value to each of these categories in order to make allowance for the maximum distance which can be traveled in each, but doing so does not correct for the differences between them in the volume of extremely short moves across county boundaries. If all counties in each state were equally populous, and if all parts of the population were equally migratory, "within-state" migrants would move an average distance of about 135 miles. Under the same conditions, migrants between contiguous states would move an average distance of about 250 miles. (These figures vary, of course, for each state and region.) In a recent study of migration within the state of Ohio, we have found that 49.3 per cent of the migrants within the state had moved within the same subregion, that is, within a small group of counties. This would indicate that the average of the distances which *are* moved is considerably smaller than the average of the distances which *would be* moved under the above assumptions.

³ The states were given a "straight line" distance equivalent, rather than a highway or railway mileage equivalent, using their approximate geographic centers for making the measurements on the maps. To simplify the calculations the states falling in zones 3, 4, and 5 from each state were grouped by geographic division. The separate zonal classification for each state was preserved in this grouping, however.

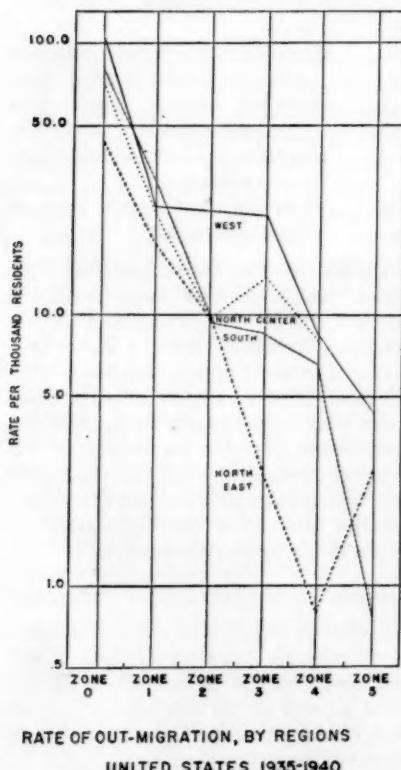
SOME DISTANCE PATTERNS OF MIGRATION

By making separate inquiries concerning rate differentials under various sets of specified conditions, it is possible to analyze several aspects of the relationship between distance and migration. Only three such inquiries will be undertaken here.

⁴ Dr. Daniel O. Price of the University of North Carolina has recently completed an excellent study which summarizes the relationship between the number of migrants and distance. The present study differs from Dr. Price's study in that it attempts to reduce the factor of distance to a continuum, insofar as that is possible; the zones have been defined for each state individually instead of for geographic divisions; separate analyses are made for within-state and between contiguous state migration; and the entire problem is stated in terms of rates and percentages rather than in terms of numbers of migrants. Dr. Price's study is more inclusive than the present one in that it includes a distance analysis of certain social and economic characteristics of migrants which as yet we have not analyzed.

Inquiry 1. Does the ability to attract migrants decrease with increasing distance?

The solution of this problem requires the use of out-migration rates, for a migrant is "exposed" to migration in the community of



(Distance scale is schematic)

CHART I

his origin, along with the other members of that community. The incidence of migration, or the frequency with which the attractive "pull" of another community or the expulsive "push" of the community of origin is successful in creating migration, may be measured by the ratio of migrants to the population exposed to migration, i.e., by the rate of out-migration.⁵ Chart I presents the

⁵This is a principle which should be carefully

rates of total out-migration from each of the four geographic regions. There can be little doubt that the distance to be traveled is a very important element in determining the frequency with which people are attracted to one of the zones. In every region the rates of within-state migration are at least $2\frac{1}{2}$ times the rate of moving to a contiguous state. The rate of moving to a contiguous state, in turn, is in every region at least twice the rate of moving to the first zone of

followed in interpreting migration rates. Rates which use the community of destination as a base (in-migration rates) do not measure the attractive power of the receiving community. Such rates measure the pace at which the local population is recruiting new members or is being "diluted" in composition by in-movement from other areas. Out-migration rates, like death and birth rates, represent the incidence of a given phenomenon among a population exposed to that class of events. For example, the population which is "exposed" to the probability of migration to California, and from which migrants are attracted to California, is the population which does not reside in California. The attractive power of California is measured, therefore, by dividing the total number of migrants which move into that state by the non-California population. Such rates are crude statements of probability. Out-migration rates to specific communities, or types of communities are, by the same line of reasoning, measures of the pull which these communities exert upon the population for which the out-rate is computed. In the present study, the rate of out-migration to each zone measures the relative attractive "pull" which each zone exerts upon the population at the zero end of the distance continuum. Even if the stimulus to out-migration is one of local expulsion rather than one of attraction from a distance, the principle that the "exposed" population is the population of the community of origin remains valid, and changes in the rates of out-migration with changes in distance represent the change in the strength of the expulsive force. In general, migration rates (both in- and out-rates) are very useful tools when the central problem is that of the part which migration plays in effecting growth or decline in populations or changes in population characteristics of communities. Percentage distributions, on the other hand, are useful primarily for comparing the migratory populations with each other or with nonmigratory populations. In computing the rates for this study, the population in 1940 has been used as a base. This is only an approximation of the "exposed" population. A precise set of rates would require as a base the population which was $2\frac{1}{2}$ years of age or older as of October 1, 1937.

noncontiguous states. In zones 2, 3, 4, and 5 the rate of out-migration tends to become smaller as the distance increases. The overall pattern is one of a very great relative decrease in rate with increase in distance. The decline of attractive power with increasing distance is so large, in fact, that the logarithm of the rate of departure, rather

Northeast and the North Center than one would have expected on the basis of distance alone. This is shown in the rise from zone 4 to zone 5 in the rate of out-migration from the Northeast, and in the rise from zone 2 to zone 3 in the rate of out-migration from the North Center. Evidently, the continued rapid development of California and other

TABLE I. RATE OF OUT-, IN-, AND NET-MIGRATION, BY REGIONS. UNITED STATES, 1935-40

Type of migration and distance	Rate of Migration per Thousand Residents, by Region				
	All Regions	Northeast	North Center	South	West
<i>Out-migration</i>					
All distances	119.5	77.7	126.1	134.8	162.7
Within-state	70.2	44.0	72.2	80.0	102.6
Contiguous state	23.9	17.9	22.7	29.8	24.8
Zone 2	8.9	9.7	9.5	9.4	— ^a
Zone 3	9.6	2.6	13.6	8.4	22.7
Zone 4	5.6	0.8	8.0	6.5	8.3
Zone 5	1.4	2.7	— ^a	0.7	4.2
<i>In-migration</i>					
All distances	119.5	73.7	114.5	128.3	226.5
Within-state	70.2	44.0	72.2	80.0	102.6
Contiguous state	23.9	17.4	23.5	27.7	30.0
Zone 2	8.5	8.1	10.7	9.1	—
Zone 3	8.7	2.6	6.3	9.6	30.2
Zone 4	6.9	0.6	1.7	1.4	54.2
Zone 5	1.4	0.9	—	0.5	9.4
<i>Net-migration</i>					
All distances	0.0	-4.0	-11.6	-6.5	63.8
Within-state	0.0	0.0	0.0	0.0	0.0
Contiguous state	0.0	-0.5	0.8	-2.1	5.2
Zone 2	-0.4	-1.6	1.2	-0.3	—
Zone 3	-0.9	0.0	-7.3	1.2	7.5
Zone 4	1.3	-0.2	-6.3	-5.1	45.9
Zone 5	0.0	-1.8	—	-0.2	5.2

^a No land area allotted to this zone.

Source: Compiled from Sixteenth Census of the United States, *Population: Internal Migration 1935 to 1940; Color and Sex of Migrants*, Table 16.

than the rate itself, tends to be a linear function of the distance traveled.

In spite of this striking inverse relationship between the rates of out-migration and mileage distance, it is apparent that the relationship is not an invariant one. Each region has its own distance patterns which is not exactly duplicated by any other region. In addition, there are some very important deviations from the general distance trend. During the 1935-40 period the states of the West exerted a stronger attractive pull upon the

western states was sufficiently attractive to induce migrants to travel greater distances to reach those areas than they were willing to travel under other circumstances. Exceptions of this type are important for at least two reasons. First, they are of sufficient magnitude to disprove the idea that there is one universal "law of distance" in migration in the United States. Clearly, factors other than those associated with distance may also play a part in influencing the rate of departure. A complete explanation of the attrac-

tive and expulsive forces requires the introduction of more than this one variable. Second, these deviations are important because they emphasize, in another way, the marked restrictive effect which distance has upon the rate of departure. When compared to the full range of values in the distance scale, the deviations noted above are relatively small. In no case is a deviation more than a small fraction of the total rate of out-migration, nor even a very large deviation from the next shorter distance in the continuum. Even among a highly mobile population such as that of the United States, the retardative effect of distance can be lessened, but apparently cannot be completely overcome, by other circumstances. The fact is that 60 per cent of the internal migrants lived within the boundaries of the same state in both 1935 and 1940, that an additional 20 per cent had merely crossed over into a contiguous state, and that only 6 per cent, or one migrant in 17, had wandered as far as 1,200 miles or more from his 1935 residence. The probability that a given person would make a "within state" migratory journey was almost three times as great as the probability that he would migrate to a contiguous state, and was ten times as great as the probability that he would journey 1,200 miles or more in changing his residence. Although the 1935-40 period was not typical of "normal" economic conditions, similar differences in the volume of movement within a state, between contiguous states, and between noncontiguous states have been shown to exist by sample enumerations made by the Census during and since the war.

While one can state no invariant law concerning distance and the intensity of the attractive forces, the conclusion is almost inescapable that the amount of distance to be traveled, other factors being held constant, is one of the factors which is closely related to the rate of leaving one point for any other point.

Inquiry 2. What part does distance of migration play in effecting a redistribution of population?

In changing their residence, migrants are

capable not only of contributing to the growth or decline of states and regions but also of changing the pattern by which the population is distributed over the land. Within recent decades the movement to urban centers, and particularly to those centers which are now defined as metropolises, has been one of the more spectacular redistributive accomplishments of migration. Although the development of new regions has inevitably required a great deal of long-distance migration, there is no such necessary relationship between the distance to be traveled and the building up of urban centers. Nevertheless, there are certain distance patterns which characterize this redistributive process.

During the 1930-40 decade, cities of 100,000 inhabitants or more and rural-farm areas gained relatively little population; other cities made a moderate growth, while rural-nonfarm areas gained population at a rapid pace. Given the ability which each of these communities possessed to grow by natural increase, in order to accomplish this particular growth combination it was necessary that the smaller urban centers and the rural-nonfarm areas make net gains by migration at the expense of rural-farm populations and cities of 100,000 and over. By computing in-, out-, and net-migration rates for each of the four types of communities it is possible to determine, in an approximate way, the part which migration played in redistributing the population toward the smaller cities and rural-nonfarm areas. If these rates are also computed for each of the distance zones, it is possible to observe the conditioning effect which distance played in this movement. Such rates are presented in Table 2.* The out-migration rates for each type of community must be interpreted with

* In making this study, a separate tabulation was prepared for each of the 16 streams of migrants between the different types of community (from each type to each type), for in- and out-migration by geographic division, sex, and color. Table 2, therefore, is a summary of 32 rather large tables. A more detailed distance analysis of the individual streams involved in movement between community types will be made in a general report on internal migration now in preparation.

extreme caution. It is known that many migrants who were living in rural areas in 1935 tended to report the nearest city as their 1935 residence. This element of mis-reporting is suspected of having been par-

stated as a conclusion to the first inquiry. Even though the rates of out-migration for urban places may be too high at the greater distances, nevertheless they follow the general pattern of decline with increasing dis-

TABLE 2. RATES OF OUT-, IN-, AND NET-MIGRATION AND PERCENTAGE DISTRIBUTION OF MIGRANTS BY TYPE OF COMMUNITY AND DISTANCE, 1935-40

Type of Migration and distance	Rate of Migration per Thousand Residents, by Type of Community				Percentage Distribution			
	Cities 100,000 and Over	Other Urban	Rural-non-farm	Rural-farm	Cities 100,000 and Over	Other Urban	Rural-non-farm	Rural-farm
<i>Out-migration</i>								
All zones	114.0	125.3	110.4	99.8	100.0	100.0	100.0	100.0
Zone 0	61.7	70.1	67.0	68.0	54.1	55.9	60.7	68.1
Zone 1	20.8	27.3	22.7	17.9	18.3	21.8	20.6	17.9
Zone 2	12.1	10.0	6.7	3.5	10.7	8.0	6.0	3.5
Zone 3	9.7	10.9	9.0	6.2	8.5	8.7	8.2	6.2
Zone 4	6.8	5.8	4.3	4.1	5.9	4.6	3.9	4.1
Zone 5	2.9	1.2	0.7	0.1	2.5	0.9	0.6	0.1
<i>In-migration</i>								
All zones	80.7	132.8	155.6	92.1	100.0	100.0	100.0	100.0
Zone 0	32.0	78.6	100.7	64.8	39.6	59.2	64.8	70.4
Zone 1	18.2	27.1	27.8	16.7	22.5	20.4	17.8	18.2
Zone 2	11.1	9.1	7.7	3.0	14.1	6.9	4.9	3.3
Zone 3	8.7	10.3	9.8	3.9	10.8	7.8	6.3	4.3
Zone 4	7.9	6.5	8.5	3.5	9.8	4.9	5.4	3.8
Zone 5	2.6	1.3	1.1	0.2	3.2	1.0	0.7	0.2
<i>Net-migration</i>								
All zones	-33.4	7.5	45.2	-7.6	-100.0	100.0	100.0	-100.0
Zone 0	-29.7	8.5	33.8	-3.1	-88.9	110.7	74.7	-40.9
Zone 1	-2.6	-0.3	5.0	-1.2	-7.9	-2.5	11.1	-15.1
Zone 2	-0.8	-0.9	1.0	-0.5	-2.3	-11.2	2.2	-6.4
Zone 3	-1.0	-0.6	0.8	-2.3	-3.1	-7.5	1.8	-29.7
Zone 4	1.1	0.7	4.1	-0.6	3.3	8.7	9.1	-8.2
Zone 5	-0.4	0.1	0.4	-	-1.1	1.8	1.0	0.4

* Less than 0.1.

Source: Compiled from Sixteenth Census of the United States, *Population: Internal Migration 1935 to 1940, Color and Sex of Migrants*, Table 16.

ticularly serious as the distance from the place of origin increased.

In spite of this element of inexactness, it is possible to arrive at some fairly definite conclusions concerning the relationship between distance and population redistribution.

1. All four types of communities are subject to the distance principle which was

tance previously noted for the total population. It would be difficult to argue that the rural populations are less subject to the restrictive effects of distance than is the urban population. It must be concluded, therefore, that the strength of the attractive "pull" or the explosive "push" upon both urban and rural communities tends to decline with increasing distance. If it were

possible to claim that the differences between the urban and the rural rates were completely genuine and not subject to the misreporting error, then it could also be stated categorically that the more urban the community of origin, the higher the rate of out-migration to a point 600 miles or more away tended to be. Because the misreporting error is present, however, not all of these differences in rate can be attributed to the type of community of origin. In the zones representing migratory journeys of 1,200 miles or more (zones 4 and 5) the rates for migrants originating in urban areas are so much higher than the rates for migrants originating in rural areas (50 per cent higher in zone 4 and more than 200 per cent higher in zone 5) that it is difficult to imagine a misreporting error serious enough to account for all of the differences. It is probably true, therefore, that at the extreme distances, the intensity of attraction upon the population of urban places is greater than that upon the population of rural areas. The percentage distributions of Table 2 leave little doubt that a higher proportion of the migrants who leave an urban place or its vicinity travel to the more distant zones than of the migrants who leave rural areas, and the greater the distance to be traveled, the greater this disparity between urban and rural areas tends to become. The cities of 100,000 or more inhabitants sent a larger percentage of their out-migrants to the more distant zones than the smaller cities, and the rural-nonfarm areas exhibited this same difference with respect to the rural-farm areas. It would appear, therefore, that the more urban or urban-like a community is, the higher the percentage of long-distance out-migrants tends to be. Taken together, these data imply that *distance restricts the movement of urban populations less than it does the movement of rural populations.*

2. It is also true that of the migrants arriving at urban places, a larger proportion have traveled long distances than of the migrants entering rural areas. Thus, 13.0 per cent of the in-migrants to central cities had traveled 1,200 miles or more, whereas only

5.9 per cent of the migrants to other urban places, 6.1 per cent of the migrants to rural-nonfarm areas, and 4.0 per cent of the migrants to rural-farm areas had journeyed this far.⁷ If a migratory journey terminated in a city, the distance traveled is more likely to have been a long one than if the journey terminated in a rural area. The more urban or urban-like a community is, the higher the percentage of long-distance in-migrants tends to be.

3. A principle which is perhaps of even greater importance than these hypotheses concerning the relative attraction of remote areas upon and for urban and rural populations is the very important part which short-distance migration plays in redistributing the population between urban and rural communities. In Table 2 the net-migration rates reveal the fact that the *urban-rural redistributive changes which were being effected by migration during the 1935-40 period were being accomplished primarily by short-distance migration rather by migration involving longer distances.* Here the element of misreporting is of less importance, for the change in the error with change in distance (rather than the absolute amount of the error) is the disturbing element in making comparisons. Eighty-nine per cent of the net loss to central cities was due to "within-state" migration, and probably reflects the continuation of the tendency of large cities to decentralize by diffusion into nearby areas. The net gain of the other urban places was due almost entirely to within-state migration. About three fourths of the large gains made by the rural-nonfarm population was due to this same type of short-distance movement. Within-state migration accounted for a higher proportion of the net loss to the rural-farm population than did migration involving any other dis-

⁷ It does not appear to be true, in this period at least, that the larger urban places tended to have higher *rates* of in-migration from great distances than other types of communities, for the rates for the rural-nonfarm population, in two of the outer zones, are higher than the rates for cities of 100,000 or more.

tance zone (40 per cent). Thus, migration within the same state accounted for not less than three fourths of the net redistribution of population into or out of cities of all sizes and the rural-nonfarm areas, and for a very high percentage of the net redistribution of the rural-farm population.

This tendency for migration within the same state to account for not only a very high proportion of all out-migration and all in-migration but also to account for a very high proportion of the net redistribution resulting from migration, is an aspect of the distance principle which is of the utmost significance for further migration research. Because state-of-birth data have provided the only comprehensive information available, and because the development of new regions has been one of the outstanding characteristics of migration for over a century, previous studies may have tended to emphasize the redistributive effects of long-distance migration at the expense of overlooking the significant changes being wrought by shorter moves. This very large volume of short-distance within-state migration should not be dismissed as not being "genuine" migration and therefore as unworthy of study. On the contrary, unless the migration between 1935 and 1940 was very dissimilar in distance structure to migration in other periods, one must conclude that future studies of urban-rural movement must concentrate upon short-distance migration. There is a definite need for a subdivision of the "within-state" migration category, which contained 60 per cent of all cases of migration in the 1935-40 period, into smaller units of area, and for studies aimed specifically at learning more about the various streams of migrants which move only short distances, and which are of prime importance in contributing to the growth or decline of urban centers.

Inquiry 3. Are there sex and color rate differentials in the distances which migrants travel?

During the period between 1935 and 1940 males proved to be slightly more migratory

than females in the Nation as a whole. (See Table 3.) The difference between the rates for the two sexes, however, is only 3 per thousand. Although the rates for males and females are almost identical for short-distance migration (within the same state), with increasing distance the rate for males becomes progressively larger than that for females. This tendency is present in each of the geographic regions. Thus, distance is less of a brake upon the movement of males than upon the movement of females.

The white population proved to be far more migratory than the nonwhite population in the 1935-40 period, although the phenomenon of decreasing rate of migration with increasing distance is exhibited by both groups. This color differential tended to be greatest at the very shortest and at the longer distances. Only in zone 2, which involved leaving the state but traveling less than 600 miles overland, was the rate of out-migration for nonwhites higher than for whites. The rate of migration within the same state was higher for the white population by 45 per cent than it was for the nonwhite population, and in zones 3, 4, and 5 the rate for the white population was 2, 4, and 5 times, respectively, the rate for the nonwhite population. Although the migration of the nonwhite population conforms to the general principle of distance derived from the first inquiry, certain additional restrictions seem to be in operation at both ends of the distance continuum. One might speculate that the social and economic situation in which the nonwhite population finds itself is such that short-distance moves are relatively ineffective in making a better adjustment and extremely long-distance moves are financially impossible.

SUMMARY

The evidence which has been assembled here indicates that there is a close inverse relationship between the distance to be traveled and the rate of migration out of an area. This relationship is not invariant, however, and distance must be accepted as being only one of several possible variables which

TABLE 3. RATES OF OUT-MIGRATION, BY DISTANCE, SEX, AND COLOR.
UNITED STATES, 1935-40

Sex and color	Rate of out-migration per thousand residents, by distance						
	Zone 0	Zone 1	Zone 2	Zone 3	Zone 4	Zone 5	All zones
Total male	70.28	24.10	9.15	10.08	5.93	1.47	121.02
Total female	70.06	23.63	8.64	9.09	5.27	1.28	117.97
<i>Ratio male rate to female rate</i>	<i>1.00</i>	<i>1.02</i>	<i>1.06</i>	<i>1.11</i>	<i>1.13</i>	<i>1.15</i>	<i>1.03</i>
White male	72.18	24.75	9.13	10.64	6.40	1.60	124.70
White female	72.74	24.32	8.43	9.58	5.69	1.41	122.17
<i>Ratio white male rate to white female rate</i>	<i>0.99</i>	<i>1.02</i>	<i>1.08</i>	<i>1.11</i>	<i>1.12</i>	<i>1.14</i>	<i>1.02</i>
Nonwhite male	53.22	18.27	9.38	5.00	1.76	0.34	87.97
Nonwhite female	47.08	17.73	10.44	4.84	1.60	0.19	81.88
<i>Ratio nonwhite male rate to nonwhite female rate</i>	<i>1.13</i>	<i>1.03</i>	<i>0.90</i>	<i>1.03</i>	<i>1.10</i>	<i>1.79</i>	<i>1.07</i>
Total white	72.46	24.53	8.78	10.12	6.05	1.51	123.44
Total nonwhite	50.10	18.00	9.92	4.92	1.68	0.26	84.87
<i>Ratio nonwhite rate to white rate</i>	<i>0.69</i>	<i>0.73</i>	<i>1.13</i>	<i>0.49</i>	<i>0.28</i>	<i>0.17</i>	<i>0.69</i>
<i>Ratio nonwhite male rate to white male rate</i>	<i>0.74</i>	<i>0.74</i>	<i>1.03</i>	<i>0.47</i>	<i>0.28</i>	<i>0.21</i>	<i>0.71</i>
<i>Ratio nonwhite female rate to white female rate</i>	<i>0.65</i>	<i>0.73</i>	<i>1.24</i>	<i>0.51</i>	<i>0.28</i>	<i>0.14</i>	<i>0.67</i>

Source: Compiled from Sixteenth Census of the United States, *Population: Internal Migration, 1935 to 1940, Color and Sex of Migrants*, Tables 17, 18, 19, 20.

condition the rate of departure from any community. Urban communities both send and receive a higher proportion of long distance migrants than do rural communities. It is also probably true that distance is less of a barrier to the migration of urban populations than it is to the migration of rural populations. The redistribution of population between urban and rural communities is accomplished primarily by short-distance migration, which is a very large proportion of all migration. There are consistent sex and color differentials associated with the distances which migrants travel: Distance is less restrictive of migration for males than for females, and less of a barrier for whites than for nonwhites. However, these differ-

entials between urban and rural communities, between the sexes, and between the white and nonwhite populations are only minor variations in the general principle that the attractive "pull" or expulsive "push" upon the population exposed to migration tends to decrease with increasing distance. These findings are all based upon migration data for the 1935-40 period, and should be verified for other periods of time before being accepted as characteristic of all migration in the United States.⁸

⁸ The Census Bureau has unpublished tabulations which would permit a distance analysis of the following additional characteristics of migrants: Age; Nativity and citizenship; Relationship to head of household; Years of school completed; Employment status; Occupation.

MORAL INTEGRATION AND INTERPERSONAL INTEGRATION IN AMERICAN CITIES*

ROBERT C. ANGELL†

University of Michigan

IN two previous papers dealing with American cities I used the term "social integration" to define the focus of study.¹ This characteristic was measured by two indexes, a welfare effort index and a crime index. Although both of these reflect the degree to which the life of a community constitutes a moral order, it was tacitly assumed that the degree of solidarity in interpersonal relations was also being measured, or at least that the latter would be closely correlated with the former. Hence it was thought that the term "social integration" was appropriate. Further research demonstrates that moral integration and interpersonal integration are two distinct things; that there is no simple phenomenon of social integration. What was previously measured was moral integration alone. This paper will give the evidence for this conclusion.

Although the objective of the large study of which one facet is here reported was to test certain hypotheses with respect to causal factors in integration, this paper is not concerned with causes. But something must be said on this subject in order to make clear why our data concern only four cities. My studies through 1946 indicated that there was great variation among large American cities in integration as measured positively by welfare effort and negatively by crime,

and that more than three-fifths of the variance was attributable to two causal factors: degree of heterogeneity of the population in terms of nationality and race; and in- and out-migration of the population. It was also found that the residual, unexplained variance in integration was much greater for some cities than for others. It was presumed that other causal factors were working strongly in the cities whose integration scores could be predicted least accurately from their population composition and mobility scores. The next step seemed to be to do intensive research on a small group of these cities, if possible some that were unaccountably good and some that were unaccountably bad. In particular it was hoped to get at socio-psychological factors in integration.

The decision was made to carry on the further work through the facilities of the Survey Research Center of the University of Michigan, which maintains a staff of trained interviewers in all parts of the country.² Because the previous data were from the year 1940 and the survey research was to be done in 1947, cities with large population changes during the war years were dropped from consideration. From the few cities remaining that were suitable in every respect, four were chosen for study—Rochester, Syracuse, Columbus, and Louisville. In the first two, other factors appeared to be operating positively upon integration, in the second two, negatively.

In each of the four cities a cross-section of the population of approximately 100 persons obtained by area sampling, and a panel

* Slightly revised version of a paper read at the annual meeting of the American Sociological Society held in Chicago, December 28-30, 1948.

† I wish to acknowledge my indebtedness to Dr. Ronald Freedman for his criticisms of the preliminary draft of this paper.

¹ "The Social Integration of Selected American Cities," *American Journal of Sociology*, XLVII (Jan. '42) pp. 575-92; "The Social Integration of American Cities of More than 100,000 Population," *American Sociological Review*, XII (June '47) pp. 335-42.

² The cost of the work of the Survey Research Center was defrayed by a grant from the Horace H. Rackham Fund of the University of Michigan. Mr. Eugene Jacobson of the staff of the Center served as Study Director.

TABLE I. THE SOCIAL INTEGRATION OF FOUR CITIES†

	Syracuse	Rochester	Mean	Louisville	Columbus
<i>General Satisfaction with the Community</i>					
1. What sort of a city is this to live in?	4.38	4.00	4.04	3.93	3.85
2. Is it a good city for raising a family?	3.86	3.93	3.73	3.33	3.80
3. Do most people think it is a good place to live?	4.90	4.60	4.78	4.88	4.72
4. Respondent believes there is a positive attitude among people toward community*	36.8%	22.1%	27.0%	21.0%	28.2%
5. Respondent would rather live here than anywhere else	61.2%	46.0%	46.1%	36.0%	41.3%
<i>Level of Personal Conduct</i>					
6. Do young people get into trouble much here?	2.59	2.97	2.37	2.09	1.83
7. Are citizens law-abiding?	4.57	4.64	4.46	4.19	4.43
8. How does it compare with other cities in this respect?	3.79	3.82	3.70	3.55	3.67
9. Overall attitude on desirability of fellow citizens**	4.21	3.95	3.96	3.90	3.77
<i>Relations among Groups</i>					
10. Is there much trouble among different races, nationalities and religions?	4.44	3.87	3.97	3.73	3.85
11. How does it compare with other cities in this respect?	3.72	3.68	3.74	3.90	3.66
12. Is there a big difference between rich and poor?	3.04	2.98	2.79	2.48	2.64
13. Respondent's own attitude toward class differences (coder's item)	3.60	3.44	3.41	3.20	3.39
14. Respondents who state that way groups get along is no problem	26.2%	12.4%	16.0%	10.0%	15.3%
15. Overall code on whether respondent thinks in terms of group conflict or not (no)	84.2%	83.2%	72.5%	65.0%	57.6%
<i>Participation in Civic Affairs</i>					
16. Do people take an interest in community affairs?	3.60	2.90	3.21	3.42	2.92
17. Overall code on respondent's interest in community	3.22	3.18	3.06	2.94	2.91
18. Do you feel that you are doing as much for the community as you want to or should you be taking a more active part?	2.53	2.59	2.56	2.64	2.46
19. Have you as much to do with what goes on here as the average person?	2.59	2.62	2.57	2.43	2.65
20. Did you vote in the last election?	65.0%	63.7%	56.1%	50.0%	45.7%
<i>Fellow Feeling</i>					
21. How do you like your neighborhood?	3.96	4.20	3.98	4.03	3.74
22. Respondent's attitude toward neighbor's (coder's item)***	3.42	3.65	3.56	3.59	3.56
23. Is this a friendly city to live in?	4.67	4.03	4.50	4.80	4.48
24. Respondent states that it is easy to get acquainted here	51.5%	35.4%	45.1%	50.0%	43.5%
Mean of 18 index items	3.72	3.61	3.57	3.50	3.46
Mean of 6 percentage items	54.1%	43.8%	43.8%	38.7%	38.6%

† Differences in index scores between any two cities are significant if .30 or more, between any city and the mean of the four cities if .20 or more. Differences in percentages between any two cities are significant if 15% or more, between any city and the mean of the four cities if 10% or more. However, the validity of the conclusions drawn rests less on the items singly than upon their cumulative confirmation of one another.

* Percentage who followed up #3 with, "people who go away come back to live," "people take pride in the city," and "people say good things about the city."

** Such overall attitudes were determined by the coders after reading the entire document on each respondent.

*** Since there was a high percentage of respondents who made no comment about their neighbors in responding to #21, the mean scores were figured two ways—by omitting the "no answer" respondents and by adding them into the middle step. The average of these two scores was taken as the score for this item.

of 25 leaders were interviewed. Data from the cross-sections only are used in this paper. Interviews were about an hour in length and covered 58 open-ended questions concerning the life of the community, its institutions, its leaders, and the attitudes of the respondent toward these matters. These interviews were coded by a carefully trained group in Ann Arbor and the results punched on statistical cards.

Although the post-war welfare effort and crime indexes that were computed for these four cities seemed to indicate that their character had not changed much since 1940,

between the columns for the two well-integrated and the two poorly-integrated cities.

The mean scores for each city on the index and percentage items confirm, in a broad way, the original rating of the cities. There is a considerable gap between the two sets of cities. Rochester, however, does not show up nearly so well as expected. Although we cannot go into the reasons for this in this paper, we will indicate later what the nature of Rochester's newly developed weakness appears to be.

The criticism may be made that the cities do not show anything like the same degree

TABLE 2. ASPECTS OF SOCIAL INTEGRATION OF FOUR CITIES

Aspect	Syracuse	Rochester	Mean	Louisville	Columbus
General satisfaction with community	4.38	4.18	4.18	4.05	4.12
Level of personal conduct	3.79	3.85	3.62	3.43	3.43
Relations among groups	3.70	3.49	3.48	3.33	3.39
Participation in civic affairs	2.99	2.82	2.85	2.86	2.74
Fellow feeling	4.01	3.96	4.01	4.14	3.93

it was an obvious precaution to check the integration rating further through the survey research. The findings reported here were one resultant of that checking process. In 1940 the differences between Rochester and Syracuse on the one hand and Columbus and Louisville on the other were very great. In the order given the integration scores were 19.0, 17.0, 8.0 and 7.7. In 1947 the corresponding figures were 18.7, 16.5, 9.1, and 6.9. Questions designed to discover whether this gap would be confirmed by the statements of respondents were included in the interview.

In Table 1 are given the survey data bearing upon social integration as it was originally conceived. For those items not recorded in percentages the range of scores is from 5.00 to 1.00. These represent questions that were coded on either a three-step (scored 5, 3, 1) or a five-step (scored 5, 4, 3, 2, 1) scale. The answers indicating the highest level of integration are scored 5 and those indicating the lowest level, 1, no matter how the question is stated. In order to facilitate the reading of the table the mean of the four cities on each question is placed

of difference on the survey data as they did on the original integration index. The reason for this is probably that many of the people in each city have no standard of comparison. The notions of what is normal are set by what they have been accustomed to in their home city. Hence their reactions will tend to distribute themselves in much the same pattern on the coders' three- or five-point scale. Any difference in mean score under these circumstances is probably quite significant.

Although Table 1 seems at first glance to validate the concept of social integration, detailed examination makes it clear that there are cross currents at work. Table 2 brings this out clearly. The scores on the first three aspects run according to expectation. In each case Syracuse and Rochester show greater integration than Louisville and Columbus. "Participation in civic affairs" shows the first inconsistency with what had been anticipated. Rochester fails to maintain its superiority to Louisville at this point. This tendency for Louisville to rise relatively appears even more sharply in the aspect of fellow feeling, where Louisville not

only surpasses Rochester but Syracuse as well. It is evident as we approach the bottom of Table 2 that factors are coming strongly into play which were not influential in the first three aspects.

It would be scientifically satisfying if we could group these new factors under the general head of interpersonal integration and let it go at that. But re-examination of the separate items in the last section of Table 1 makes it clear that there is lack of consistency within the area of fellow feeling. Syracuse is a friendly city, but people do not like their neighborhoods very well, and like their neighbors even less. Rochester, on the other hand, shows up well both as to neighborhoods and neighbors, but relatively poorly with respect to city-wide friendliness. Louisville is very strong in friendliness and at least average in the other two respects. Columbus is very near the mean of the four cities on all items except that concerning the neighborhood, where it is quite inferior. What can we make of this seemingly contradictory set of data?

On many of the questions the respondents were asked to give their reasons for the statement made. Item 24, for instance, is one reason given in following up the answer to the question of item 23. Fortunately, in connection with item 21, the respondents were asked what they liked and what they did not like about their neighborhoods. These answers show clearly that many of them were thinking primarily of the physical characteristics of the neighborhood—its cleanliness, accessibility, etc.—rather than its social attractiveness. Hence the answers to this question are not a sound index of interpersonal integration.

Let us now examine the comparison between items 22 and 23. Columbus is mediocre in both respects and Louisville is mediocre on the first and superior on the second. But Syracuse and Rochester show opposite tendencies—the former to have city-wide friendliness but inferior neighborly feeling and the latter to have friendliness toward neighbors but inferior city-wide feeling. It is evident, then, that these two items are

not measuring the same thing, that when people express themselves as liking or disliking their neighbors they are not necessarily giving their opinion of the friendliness of the city. The interviews themselves confirm this clearly. A friendly city is interpreted as one where one finds friendliness in many relations—on the part of the clerks in stores, the fellow passengers in street cars and buses, those whom one meets in parks or at P.T.A. meetings. Syracuse is rated high in this respect but Rochester appears to be socially segmented. This diagnosis is confirmed in another part of the study where Syracuse leaders and the cross-section are shown to have much the same attitudes on civic problems and organizations, whereas the Rochester leaders and cross-section are widely split in their attitudes.

Since item 24 is a derivative of item 23, the best index of city-wide interpersonal integration is item 23, "Is this a friendly city to live in?" Our problem is to determine how this relates to the other dimension, from now on called moral integration, which is apparently operating all through the upper part of Table 2.

It is appropriate to insert at this point a brief statement regarding the concept of moral integration. As we are using it, it is very similar to Durkheim's mechanical solidarity and to what Cooley called moral unity. A simple definition would be: the degree to which the life of the group proceeds in terms of shared ends and values. The life of the group will obviously not proceed in these terms unless persons develop a sense of mutual responsibility for each other and a loyalty to the institutions of the group.

Since, if cities are taken as the unit, there would be only four cases on which to compute a correlation coefficient, I decided to make the study of relationship in terms of the 408 individuals comprising the cross-section in the four cities. At the same time I determined to try out some hypotheses regarding the analysis of qualitative variables that had been developed by Dr. Clyde H. Coombs of the University of Michigan Psy-

chology Department. This made it necessary that I select a number of items to represent the factor of moral integration, each of which was coded in the same number of steps (in this case, three), and it made it desirable that the mean scores of these items be separated from one another by wide intervals. The items chosen, their mean scores for the four cities, and the weighting coefficients which were used to give the five scores roughly equal effect upon the total score of an individual are presented in Table 3.

The summation of the weighted scores of a single person on these five items could be related to his response on item 23, "Is this

TABLE 3. COMPONENTS OF THE MORAL INTEGRATION INDEX

Item No.	Item	Mean Score	Weighting Coefficient
3	Do most people think it is a good place to live?	4.78	4
7	Are citizens law-abiding?	4.46	5
10	Is there much trouble among different races, nationalities and religions?	3.97	6
17	Overall code on respondent's interest in community	3.22	7
6	Do young people get into trouble much here?	2.59	8

a friendly city to live in?" This made possible the computation of a correlation coefficient for each city and for all four cities as a single universe. These are given in Table 4, and the mean of the correlation coefficients of the four cities is added for comparative purposes.

It is apparent from the coefficients given that the lines of regression of the several cities have different slopes, since the coefficient for all four cities as a single universe is less than that of the coefficient for the city showing the least correlation. It is also clear that the relation between the two variables in the morally well integrated cities is less significant than in the poorly inte-

grated cities. If it is assumed that this difference reflects a difference in the objective situation in the two sets of cities, then we must conclude that the social processes in Louisville and Columbus are less differentiated than in Syracuse and Rochester, that the concept of overall social integration is

TABLE 4. CORRELATION COEFFICIENTS OF MORAL INTEGRATION AND INTERPERSONAL INTEGRATION

All four cities as a single universe	.29
Syracuse	.34
Rochester	.31
Louisville	.48
Columbus	.57
Mean of four cities	.43

more applicable to them. If, on the other hand, it is assumed that the difference is in the discrimination of the respondents themselves, then we would conclude merely that the members of the cross-section in Louisville and Columbus are less competent to analyze the objective situation. It is probable that there is some truth in both of these hypotheses. In any event the general fact is clear that interpersonal integration and moral integration, in so far as we have successfully measured them, are fairly distinct phenomena in large American cities.

The data with respect to the five components of the moral integration index enable us to press further with the analysis of that aspect of city life and to try out some of Dr. Coombs' ideas. Of six scores which he computes from data such as ours, only three will measure aspects of the cities' moral integration. These are what he calls the group status score, the group dispersion score, and the trait status score. The first of these is given by the mean score of the persons in each city, the second, by the standard deviation of their scores. The trait status score is more complicated. It bases back on a fourth score he uses, the individual dispersion score, and is merely the mean of such scores for each city. We must therefore get a clear idea of what the individual dispersion score is. This may be done by referring to the five items of Table 3. It is obvious that if

the life of the city reveals the same general pattern to a given individual as it does to the great majority, he would tend to respond more positively to the question, "Do most people think it is a good place to live?" than to the question, "Are the citizens law-abiding?" Supposing him to be an accurate observer of what is going on around him, a more positive response to the second question than to the first would indicate that the slice of the city's life to which he is exposed is differently structured from that to which others are exposed. This argument should hold for every pair of items in Table 3. It is possible, then, to compute what Coombs calls a dispersion score, or what I would

agree more closely than do the residents of the other cities on the overall level of moral integration of their community. Although Columbus is poorly integrated, Columbus people agree in their judgment to a greater degree than do the people of better integrated Rochester. Louisville and Rochester appear to be the most socially segmented cities. A slightly different facet of the problem is given by the trait status scores. These indicate the extent to which the answers to the five questions are so patterned that they may be interpreted as referring to a single dimension, valid throughout the city. On this criterion Syracuse comes off best, with Columbus next.

TABLE 5. MORAL INTEGRATION SCORES FOR FOUR CITIES

Index	Syracuse	Rochester	Mean	Louisville	Columbus
Group status score (mean of individual scores)	113.0	110.9	106.2	100.5	100.5
Group dispersion score (standard deviation of individual scores)	18.3	20.4	19.7	21.0	19.2
Trait status score (mean of individual dispersion scores)	14.3	22.2	18.3	18.9	17.8

prefer to call an inversion score, for each person. It is the percentage of times that his observations dictate a different relative emphasis among the five items from that usually given. Note that this percentage is quite independent of whether he judges the city favorably or unfavorably. It is the relative ordering of his judgments that counts. If there are no inversions, his dispersion score is zero whether his scores on each question are high or low. If now we take the mean of these dispersion scores for each city, which is the trait status score, we obtain a measure of the degree to which moral integration in a given city is a single dimension. The smaller the trait status score, the more nearly does the moral pattern throughout the differentiated areas in which the individuals of the cross-section live have a uni-dimensional character. Table 5 gives the pertinent scores for the four cities.

The group status scores merely confirm what we already know about the moral integration of the four cities. The group dispersion scores show that Syracuse people

The general conclusion from these measures for each city may be expressed as follows: Syracuse is well integrated morally, its people tend to agree on the city's level of integration, and moral integration tends to be a single dimension. Rochester is well integrated morally, but its people show considerable variation in their judgment of the matter, and moral integration tends not to be a single dimension at different social levels or in different areas. Louisville is poorly integrated morally, its people tend to disagree about it, and moral integration tends not to be a single dimension throughout the community. Columbus is poorly integrated morally, there is an average amount of agreement concerning its level of moral integration, and moral integration is unidimensional to an average degree.

If now we refer back to the results on city-wide interpersonal integration as revealed by the answers to "Is this a friendly city to live in?", we find interesting relationships. Syracuse, Rochester, and Columbus square with common sense. In Syracuse

friendliness goes with agreement about the level of moral integration, and in Rochester comparative unfriendliness goes with lack of agreement. In Columbus more or less average friendliness is matched by average agreement on the level of integration. But Louisville presents a flat contradiction to the trend in the other cities. Here friendliness goes with lack of agreement about the city's moral integration. This finding casts serious doubt on the validity of the high friendliness score of Louisville. Several colleagues have suggested that the Louisville answers to item 23 represent a Southern stereotype rather than reality. They call attention to the traditional emphasis in the South on matters which might create such a stereotype, like hospitality, social ritual, even paternalism. But they doubt that these traits function much to produce real friendliness among the elements of the population in a city so large and heterogeneous as Louisville. Another possible explanation would seem to be—and perhaps this is another way of stating the same thing—that friendliness is a more superficial trait in Louisville than in the other three cities and does not lead to exchange of views on those matters that are related to a city's moral integration.

I am myself inclined to favor still a third explanation. A reading of the interviews in

Louisville has convinced me that the white and Negro groups there live so largely in their own social worlds that the respondents did not refer the question, "Is this a friendly city to live in?" to the city as a whole, but to their special segment of it. Hence the high friendliness score of Louisville would not contradict the hypothesis that agreement about moral integration and degree of friendliness are positively related, because Louisville's friendliness score is spurious. Two social worlds with high friendliness within each of them is perfectly compatible with disagreement over the degree of moral integration of the city.

If this explanation takes care of the seemingly exceptional case of Louisville, we can formulate our conclusion as follows: Though moral integration and interpersonal integration are two different dimensions, the degree of agreement on the level of moral integration among the people of a city is a function of the degree of city-wide interpersonal integration. This appears to be a reasonable relationship because the greater the interpersonal integration, the wider the common basis of experience. Unfortunately our evidence is not as yet extensive enough to claim that this generalization has been proven. It constitutes, rather, an inviting hypothesis for further research.

SOCIAL PARTICIPATION IN RELATION TO SOCIAL DISORGANIZATION*

STUART A. QUEEN

Washington University

ASSUMPTIONS AND HYPOTHESES

IT IS frequently assumed by leaders in our democratic society that effective organizations of the purposive sort depend on general participation by their members. Thus campus leaders urge their fellow students to take more part in "activities."

* Paper read before the annual meeting of the American Sociological Society held in Chicago, December 28-30, 1948.

Politicians urge members of the party to attend rallies, ring door bells, and watch the polls. Trade union officers are concerned about members paying dues, obeying rules, presenting a united front during a strike, rewarding their friends and punishing their enemies. Scout masters emphasize the daily good deed, winning merit badges and taking part in "camporees." Clergymen stress attendance at religious services, marrying within the faith, assisting coreligionaries,

supporting denominational institutions, etc., No matter how self-sufficient leaders may feel in making programs and decisions, giving instruction and advice, they commonly give explicit recognition to the assumption that active participation by members is essential to achievement of group ends, if not to group survival itself.

Some work done by sociologists and others indicates that this assumption is not without foundation. The publications of Kurt Lewin, Grace Coyle, Saul Alinsky, Leroy Bowman, and others provide some of the evidence concerning group success and individual participation.

The study and practice of "community organization" indicate that maintenance of a neighborhood or a community as a social group is closely related to the active participation of neighbors and fellow citizens. I refer here to studies of the U.S. Department of Agriculture (Division of Population and Rural Welfare), colleges of agriculture, Community Councils and Chests, Inc., the Cincinnati Social Unit experiment of 30 years ago, and many others which these will suggest. The research of sociologists and the practice of social workers indicate that the preservation of these locality groups is definitely bound up with the active participation on their constituents.

Some specific studies bearing on this problem are those of Mangus and Cook in Ohio, Lindstrom in Illinois, Anderson in New York, and Wakeley in Iowa. In fact, the very definitions of neighborhood and community imply, if they do not specify, the sharing of locality, tradition, interest, and activity.

A second assumption of leaders in democratic society and students of group life is that persons who are active in one group are usually active in others as well. It is almost a commonplace observation that if you want something done for an organization or a community you should give the job to someone who is already busy. Leaders and workers in churches, community chests, farm bureaus, student government, political parties, and scientific societies are generally found to be men and women who are active

in several groups.

Supporting evidence is found in Ray Wakeley's article in *Sociometry*.¹ It appears in the point systems adopted at various colleges and universities to guard against the concentration of student officeholding in a few hands. It is found in the advice offered by the *Social Work Year Book* to the effect that "Effective boards . . . should, through rotation or other means, enlist the service of active and outstanding community leadership."² All this implies that there is a wide range of differences in social participation and that persons whose degree of participation is high are especially important for the survival and success of many groups and institutions.

A third proposition for which we have some evidence is that situations in which social participation languishes are also those in which "indices of social disorganization" are high. We refer here to indices of poverty, delinquency, disease, broken homes, mental disorder, suicide, etc. For example, a study made in Kansas City some years ago involved comparison of two districts very much alike as to ethnic composition, economic status and schooling, but very different as to incidence of cases in juvenile court, clinics, and relief agencies. A concomitant variable was membership in organized groups such as clubs, trade unions, and churches. To be sure, there was also another significant variable, namely, residential mobility; so that it is difficult to separate the influence of mobility from that of participation in groups.

We have additional, though still incomplete evidence in the negative relationship between the distribution of the so-called pathologies and of formal schooling. The latter we have found to be positively correlated with social participation scores to be discussed later. Ecological studies in various cities show clearly that poverty, vice, crime,

¹ Ray E. Wakeley, "Selecting Leaders for Agricultural Program," *Sociometry*, X (Nov. 1947), 384, 395.

² Russell H. Kurtz, ed., *Social Work Year Book*, 1947, New York: Russell Sage Foundation, 1947, p. 553.

and disease abound in areas where the average amount of schooling is low, and where by inference general social participation is low.

All these data taken together with others of similar character seem to lend strong support to the hypothesis that social participation is negatively related to social pathology and social disorganization, at least so far as spacial distribution is concerned.

The question now arises: is there any relation between social participation and personal difficulties in the experience of individuals. Faris and Dunham have in various places presented evidence that social exclusion or isolation contributes to certain types of mental disorder, especially schizophrenia. Some more limited studies by students at Washington University have shown that very low social participation for a long period before hospitalization is characteristic of certain mental patients. It also appears that the relation between mental disorders and social participation operates in reverse or in a circle, for such breakdown serves to exclude the victim from many types of social and cultural activity.

The relation of social participation to poverty seems fairly clear, although the evidence is less precise than we would like. Large numbers of persons combine in their individual experience low income, restricted participation in organized groups, little neighboring, little schooling, little reading, little use of museum, theatre, concerts and other cultural facilities. Supporting data may be found in the second edition of our *Social Pathology*, in Warner's study of *Yankee City*, Davis and Gardner's *Deep South*, Mangus and Cottam's *Farm Population and Rural Life Activities*, and many others.

With reference to crime the evidence is not so clear. There is no reason to suppose that the amount of social participation among "professional" criminals, "white-collar" criminals or juvenile offenders is lower than that of their law-abiding fellows. Indeed, in a comparison of 100 inmates of the Indiana School for Boys with 100 non-delinquents, matched as to age, color, and nativity of parents, Atwood and Shideler

found that the social participation scores of the delinquents were even higher than those of the non-delinquents.³ But it appears that such habitual offenders as vagrants, alcoholics, drug addicts, and petty thieves are relatively isolated persons. Moreover, there is evidence that prejudice against ex-convicts and "jail birds" helps to exclude them still further from participation in many groups.

We might go on to consider hypotheses and the limited data available concerning other personal difficulties and social participation—divorce, illegitimacy, unemployment, prejudice against transients and members of ethnic minorities, etc. But enough has been said to make it plain that here are some important *problems*, some likely *hypotheses*, and some initial evidence. What we need next are dependable *measures* of social participation.

ATTEMPTS TO MEASURE SOCIAL PARTICIPATION

Fortunately some work has already been done and we have some scales that deal with certain aspects or varieties of social participation. One of the first was the Franklin participation-rating scale devised by Atwood and Shideler. It includes three categories.

1. *Formal* participation, including: school, school clubs and athletic teams, church and church clubs and athletic teams, Sunday School, Boy Scouts, Y.M.C.A., and work groups.

2. *Informal* participation, including: boy chum, girl friend, games such as tennis, billiards and pool, dance groups, and plays, pageants and contests.

3. *Derivative* participation, including: movies, radio, newspapers, books and magazines.

Not only types, but also extent of participation was taken into consideration. In the case of formal and informal participation, measurement included such factors as the person's position in the group, the average number of times meetings were attended and the time devoted during the week to the organization and activity.

³ B. S. Atwood and E. H. Shideler, "Social Participation and Juvenile Delinquency," *Sociology and Social Research*, XVIII (May-June, 1934), 436-441.

In the case of derivative participation, measurement included the factors of types of reading material, time spent in such reading, and the number of attendances at the movies.

Each of the items on the schedule was weighted in accordance with the degree of influence that it was believed to exert on the personalities of young people of the ages 12 to 16 inclusive. Weights were arbitrarily assigned to each item by six judges of this nature. Averages of the resulting weights for each item were assigned for each of the forms of participation.⁴

Another is Chapin's 1937 scale, which deals satisfactorily with participation in organized groups, but with no other social activity. This scale is no doubt known to most members of the American Sociological Society. More recently Donald Hay has devised a scale for the measurement of social participation of rural households.⁵ This device includes items similar to those in the Chapin scale and adds: number of families visited regularly; and informal group activities such as picnics, card games, movies, dances. Hay's scale thus takes account of types of social participation not covered by Chapin. Part of it might be applicable to any English-speaking population, but part of it deals specifically with rural households.

Because of the limitations of scales so far developed, we at Washington University set to work several years ago to devise something which we hoped might be more adequate. Several different schedules were tried out before we finally settled on the one now in use. This one is divided into four parts: A. Participation in Organized Groups, B. Cultural Participation, C. Neighborhood Participation, D. (Other) Informal Social Participation.

Section A is a direct borrowing of Chapin's 1937 scale, with his permission. The only variation is that we apply it to individuals, not to married couples.

Section C is a modification of the neighboring scale devised by Jessie Bernard in 1935. We selected and reworded 8 of her

25 questions, and we revised her method of scoring. For example, she asked, "About how often do you chat or visit with your neighbors?" She checked, "Never, Rarely, Sometimes, or Often." She scored zero, if the answer fell to the left of the median, and one, if it fell to the right. We asked, "About how often do you chat or visit with people in your block?" We checked "Once a day or more, At least weekly but not every day, At least monthly but not every week, Never." We scored for the corresponding answers: 3, 2, 1, 0.

Section D has to do with informal social participation beyond the immediate vicinity, defined for the purposes of our study as the block. This section is included because obviously the social relations of modern city dwellers are often scattered over a wide area. Hence we inquire about visits to and from relatives, friends and acquaintances, giving and attending parties, telephone calls, personal letters. We might have asked many more questions, but for practical purposes of interviewing we found it necessary to keep the thing within bounds. Also it appeared in the pre-testing that these activities were representative of those engaged in by a wide variety of persons.

Section B, Cultural Participation, has to do with the more impersonal aspects of the social environment. Here we ask about regular use of radio, phonograph, telephone, car; books read in the last month; magazines and newspapers read regularly; number of times in the last month that respondent attended: movies, stage shows, musical events, study groups, athletic contests, museums and exhibits. The scoring of the responses was quite arbitrary. In the last group of items we assigned one half point for each event attended. For each book read we assigned two points. Other items were given one point each. We frankly do not know whether this *ad hoc* weighting is sound or not.

One other section might have been added. You will note that we have not covered family life, which is for most people a major area of social participation. The omission was deliberate and rested on our belief that the family is so different from the other areas,

⁴ *Ibid.*, 436-437.

⁵ Donald G. Hay, "A Scale for the Measurement of Social Participation of Rural Households," *Rural Sociology*, XIII (Sept. 1948), 285-294.

and deserves such detailed study that it ought to be treated separately.

Up to this point, then, we had developed what amounted to four distinct schedules, each with some plan for scoring. Our text problem was how to combine them. After much trial and error the method we adopted was this. We took our principal sample of 1,500 schedules and translated the scores of each section into percentiles. Then we added the percentile ratings on each schedule and divided by four. This was based on the assumption, admittedly not demonstrated, that each section was of equal importance. That being presumably true, it was necessary to correct for the widely varying maxima in the four sections. For example, the maximum possible score in Section C is 25; in Section D, it is 33; in Sections A and B there are theoretically no upper limits, the actual maxima in our sample being 93 and 88 respectively.

Now we come to the inevitable questions of validity and reliability. Sections A and C have been tested in their present or in closely related forms by Chapin and Mrs. Bernard. Sections B and D clearly deal with other

forms of social participation. The negative intercorrelation between scores on the four sections indicates that each deals with something distinct. But we frankly do not know with reference to Section B whether the results would be seriously modified by using different items or assigning different weights. As partial checks on the validity of our schedule of scored items we asked each respondent two questions: What percentage of an average day is spent with other people? 0-25, 26-50, 51-75, 76-100. What is the approximate number of persons interacted with each day (include all persons with whom there is conversation of any kind)? 0-5, 6-25, 26-50, 51-100, over 100. The following tables show that the hope that our scheme had validity was supported by answers to the first question and at least not negated by answers to the second.

We have not yet had time to test reliability by the method of split halves. We have not found it practicable to reinterview our respondents. These matters are still unfinished business.

What we have actually done is this. After

PER CENT OF DAY REPORTED AS SPENT WITH OTHER PERSONS IN
RELATION TO COMBINED SOCIAL PARTICIPATION SCORES

Combined Scores, Quartile	0-25%	26-50%	51-75%	76-100%	No. Resp.	Total
First	169	82	67	57	3	378
Second	108	84	101	82	4	379
Third	86	121	71	87	2	367
Fourth	51	74	135	113	3	376
Total	414	361	374	339	12	1500

PER CENT OF PERSONS INTERACTED WITH EACH DAY COMPARED
WITH COMBINED SOCIAL PARTICIPATION SCORES

Combined Scores, Quartile	0-5	6-25	26-50	51-100	100+	No. Resp.	Total
First	129	124	60	25	35	5	378
Second	68	140	78	50	36	5	377
Third	37	152	76	66	36	1	368
Fourth	31	127	87	78	49	5	377
Total	265	543	301	219	156	16	1500

preparing the schedule and a set of instructions we trained selected students in interviewing. We assigned each student to a definite district so as to cover the 128 census tracts of St. Louis. We checked the returns against the 1940 Census figures as to sex, age, race, marital status, and schooling. We stimulated the interviewing of more Negroes and of more men when these were running below the indicated proportions. The following tables show the degree of our success in obtaining a fairly representative sample.

Per Cent Distribution of Sample

	RACE	
	Sample	General Population
White	82.8	87.1
Negro	17.2	12.9
	SEX	
Women	49.7	52.7
Men	50.3	47.3
	MARITAL STATUS	
Single	17.7	17.3
Married	71.5	65.3
Widowed	7.3	14.0
Divorced & Separated	2.7	3.4
	AGE	
20-29	22.9	23.6
30-39	23.2	23.9
40-49	28.0	20.6
50-59	16.2	15.8
60-69	5.7	10.1
70 and over	4.0	6.0
	SCHOOLING OF WHITES	
Years		
0	1.2	2.2
1-4	3.7	8.4
5-8	33.3	58.5
9-12	36.6	22.0
13-15	14.8	3.5
16 and over	0.5	0.8
	SCHOOLING OF NEGROES	
Years	Sample	General Population
0	0	4.2
1-4	8.1	19.3
5-8	40.5	52.7
9-12	39.0	18.3
13-15	8.1	2.6
16 and over	3.1	2.1

The foregoing tables indicate that in regard to race, sex, age, and marital status

our sample was fairly representative. With respect to schooling, we had too many respondents with high school and college education and too few with little or no schooling.

It was mentioned earlier that the scores in the four sections of our schedule surprised us by the lack of positive correlation. We rather expected that people who are active in organized groups might have little time for neighboring, but we anticipated that the "joiners" would have high scores in cultural participation. At all events when we computed the intercorrelation of scores in the four sections we obtained only negative coefficients.

COEFFICIENTS OF CORRELATION	
Sections of Schedule	Pearsonian <i>r</i>
A & B	-.29
A & C	-.18
A & D	-.30
B & C	-.16
B & D	-.39
C & D	-.35

Just in passing, it may be of interest to note the relation between some other factors and the combined social participation scores. Length of residence in a district and amount of schooling were directly and positively related to social participation. Almost the reverse was true of age. There was not a large difference between men and women or between married and single persons. As to race, there was less difference than we had anticipated. Whites were higher on cultural participation, Negroes on neighboring. On the Chapin scores white men were highest and white women lowest, Negroes being intermediate. Both races and both sexes were very much alike in informal activities.

There remain many unanswered questions about the scheme we have proposed for measuring social participation. It neglects what might be called "intensity" of social relationships. It does not differentiate between relationships of acceptance and those of hostility, activities which are constructive and those which are disrupting to group life.

But we should not conclude this discussion without pointing out again the possible uses of some social participation scale in studying

social disorganization. With such a scale we have a new approach to understanding the difference between conforming and aberrant members of a group. We may test the hypotheses set forth at the beginning of this paper concerning social participation of individual members and the survival or success of a group. We may use this device to measure the success of a program for the social

rehabilitation of ex-convicts, convalescent diabetics, unmarried mothers, displaced persons and many other marginal persons. We may use it to compare total populations in depression, war, industrial unrest and so-called normal times. All in all, it seems that here is a device that should prove very useful in studying many aspects of social disorganization.

THE GROUP APPROACH TO SOCIAL REINTEGRATION*

MARSHALL B. CLINARD

University of Wisconsin

ONE SUGGESTION for a unique point of view for the sociologist interested in social disorganization is that he should study and analyze value conflicts in a society.¹ The possibilities for such an integrated sociological approach appear encouraging. Queen has suggested that the definition of the field as well as research should be carried on within the framework of social participation.² Fundamentally, the role of the sociologist working in social and personal disorganization should be not only analysis of formal and informal group structures but of the relation of the person to social groups.

Despite their previous training and the need for a unique point of view, sociologists working in the field of social disorganization have largely neglected to place research em-

phasis on social groups.³ Some research actually represents historical investigations, legal surveys or journalistic accounts of human experience without utilization of a unique point of view. Much of the research work is still concentrated on the individual, on multiple factor studies such as the tabulation of statistically convenient items in connection with prisoners, probation or parole prediction studies, ecological studies, and the like, studies which may represent excellent demonstration of techniques but whose factors are seldom meaningful in terms of human behavior, group behavior, or to a theory of social disorganization. Rather than getting at the subjective meaning of the world to the deviant and at the nature of group attitudes and integration, the typical so-called prediction study, for example, consists chiefly of an analysis of a long list of easily devised taxonomic factors.⁴

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¹While this concept is implicit in Thomas and Znaniecki's *The Polish Peasant in Europe and America*, see particularly the writings of Frank, Waller, Fuller, and lately John F. Cuber and Robert A. Harper, *Problems of American Society: Values in Conflict*, New York: Henry Holt & Co., 1948.

²Stuart A. Queen, "The Concepts of Social Disorganization and Social Participation," *American Sociological Review*, VI, (June, 1941), 307-316. Also see Stuart A. Queen and Jennette R. Gruener, *Social Pathology*, New York: Thomas Y. Crowell Co., 1940. Queen, by extending this concept beyond personal group participation, uses this concept much more broadly than implied in this paper.

³The current emphasis on industrial sociology represents in part the discovery by many sociologists that interaction in social groups is important for the understanding of human behavior.

⁴Although most prediction studies seldom take into account such factors, in a study of recently released inmates of a reformatory, men who had never been in such an institution before, we have found considerable evidence, as one might expect, of variations in group integration. Many of the old ties had been broken or forbidden, and there was fear of the neighbors' knowing of their incarceration, stories having been invented about their absences. In some cases families had moved or they had no home; in other cases the family even ostracized

There is, moreover, an increasing tendency to accept claims, without adequate supporting evidence, that forms of deviant behavior such as functional mental disease and chronic alcoholism, are primarily an outgrowth of personality traits developed largely in a single group experience, namely early family association, and that interaction in this one group virtually supersedes all subsequent interaction in the person's life. This is not to say that there is not already considerable evidence to indicate the importance of childhood family experience, and the role of psychogenic traits in human behavior, but that the development of an adequate theory of social and personal disorganization as well as the solution of pressing treatment problems needs further research into the nature of social interaction within all groups, both in childhood and adult life.

There have been numerous developments, primarily during the past ten years, involving the application of group methods in the prevention and treatment of social and personal disorganization. Such efforts are not only suggestive as applied situations in developing sociological theory, but also represent the possibility of a vocational opportunity for sociologists. In most instances sociologists have not been involved in these group experiments, either in therapy or research; in others their relationship has been numerically insignificant. This work has included efforts of experts working with groups of persons to overcome difficulties which have been known in various forms as group therapy, clinical group work, therapeutic group work, group psychotherapy, sociometrics, and psychodrama. Other work has consisted of neighborhood councils, or of activities like Alcoholics Anonymous, Recovery for mental patients, and lately Addicts Anonymous in which expert guidance has been kept at a minimum. Still other work,

them, and generally they had lost many of their previous friends and had difficulty in securing new ones. Many were alone in the world, faced by stigma and filled with bitterness. "Family and Community: The Ex-Inmate's View," unpublished manuscript.

such as that of Lewin and his Research Center for Group Dynamics, has been a combination of theory and application in the study of group dynamics in problem areas,⁵ and certain group experiences of members of the armed services have contributed valuable information to the understanding of the role of group integration in the behavior of the individual. These efforts to apply the orientation of the group have ranged over areas such as neuroses and psychoses, delinquency and criminality, alcoholism, adolescent problems, family problems, old age adjustments, as well as racial and religious prejudice.

In the field of mental disease it is conceivable that the explanation may be found not only in certain trait structures but in the nature of social integration and interaction within social groups. The research of Faris has indicated that certain neuroses and schizophrenia may represent social isolation from group association.⁶ Isolation of old persons from group participation may be significant in producing the senile psychoses. It has even been suggested that manic-depressive psychoses are produced by extremely intimate and intense social contacts.⁷ Some validation of these views was revealed by the relation of group integration to neuroses in the armed services during the recent war. There it was learned that integrating or non-integrating forces immediate in the social environment around the individual were far more important than either the personality make-up of the individual, his personal maladjustment, or an examination of his family structure. The presence or absence of group supportive elements, particularly identification with a group under conditions of stress, was found to be one of the most important keys to the development of

⁵ Kurt Lewin, *Resolving Social Conflicts*, New York: Harper and Brothers, 1948.

⁶ Robert E. L. Faris, "Cultural Isolation and the Schizophrenic Personality," *American Journal of Sociology*, 40, September, 1934. Also his *Social Disorganization*, New York: The Ronald Press Co., 1948, Chapter 8.

⁷ Robert E. L. Faris and H. Warren Dunham, *Mental Disorders in Urban Areas*, Chicago: University of Chicago Press, 1939, p. 173.

mental disorder even in those with little supposed tendency in that direction.⁹ One commission of civilian psychiatrists who studied combat neuroses found that "When an individual member of such a combat group has his emotional bonds of group integration seriously disrupted, then he, as a person, is truly disorganized. The disruption of the group unit is, in the main, a primary causal factor, not a secondary effect of personal disorganization."¹⁰ As the psychiatrist, William Menninger, puts it, "We seemed to learn anew the importance of the group ties in the maintenance of mental health. We were impressed by the fact that an individual who had a strong conviction about his job, even though his was a definite, unstable personality, might make remarkable achievement against the greatest of stress."¹¹ Such information, while limited, suggests that it might be well to analyze similar situations in civilian life which cause mental breakdowns.

Group psychotherapy, the current emphasis having chiefly grown out of the physical impossibility of treating cases individually during the war, is suggestive for sociological research on mental disease. The usual method is for a psychiatrist and from six to twenty patients to conduct frequent discussions as a group in which there is group sharing of experience. Sometimes additional tools such as the psychodrama are employed where conflict situations are acted out in a group. These efforts have a history of development in many directions, including the work of Pratti with tubercular patients, of Marsh, Wender and Lazell in mental hospitals, of Moreno and his associates in the

psychodrama, and of Schilder, Redl, Slavson and Klapman on theory.¹²

Without subscribing to the often weird symbolism and extreme theoretical views held by some, or the overly psychiatric emphasis in general in these experiments, the approach appears to be not only sociologically sound but to furnish significant research possibilities. While no carefully controlled experiment has as yet been made of the results of group therapy, there is an almost unanimous opinion, among those who have been engaged in this work, that group therapy is effective.¹³ Although little fundamental research has been done on what takes place in such group sessions, it seems possible that the encouraging results obtained are due not to the theoretical scheme of the group analysis but to informal group adjustment. Certainly the work of Alcoholics Anonymous, without the presence of a psychiatrist, would indicate this. In group psychotherapy the members appear to develop an identification with one another and a degree of group integration, sometimes the opinion of the group appears to change the personality pattern and attitudes of one of its members, and each member secures an opportunity for new roles and a new conception of himself. In the light of the problems of others it is possible for the patient to see his own difficulties and relieve his feelings of social isolation.¹⁴ The principal contribution to mental patients appears to be in the modification or elimination of egocentricity and social isolation.¹⁵ The sociologist willing to do research in this field

⁹ William C. Menninger, "Psychiatric Experience in the War, 1941-1946," *American Journal of Psychiatry*, 103: 577-586, March, 1947. See also his *Psychiatry in a Troubled World*, New York: The Macmillan Co., 1948, Chapters V and VI, and S. Kirsner Weinberg, "The Combat Neuroses," *American Journal of Sociology*, 51: 465-78, March, 1946.

¹⁰ L. H. Bartemeir, L. S. Kubie, K. S. Menninger, J. Romano, and J. C. Whitehorn, "Combat Exhaustion," *Journal of Nervous and Mental Diseases*, 104: 370, October, 1946.

¹¹ William C. Menninger, "Psychiatric Experience in the War, 1941-1946," *op. cit.*, p. 581.

¹² S. R. Slavson, *op. cit.*, p. 1.

and approach it with his background of training and insights, may conceivably expect to contribute not only to a theory of disorganization but to therapy as well. As one psychiatrist states the problem, "It is the group itself that becomes the therapeutic agent as a result of the interaction between the individuals who form the group. It is the lack of knowledge of the dynamics of the group that at present limits the extent of the new therapeutic procedure."¹⁵

Some procedures also suggestive for sociological research have gone beyond the restricted approach of group therapy to methods of reintegrating the mental patient under more normal group situations. The treatment of the mentally ill at Gheel, Belgium, for example, consists chiefly of a procedure whereby some 3,000 patients are incorporated into a small city, living as part of the community rather than under the general method of institutionalization. The method of placing patients in individual homes has also been established in several other European countries, and while the number is relatively small, it is estimated that some 7,000 patients are receiving family care in some ten states in this country. The results, while not conclusive, appear to be quite successful.¹⁶

What we have suggested here in these various group treatments of mental disorders is that the etiology may not lie primarily in the conventional individualistic, early childhood explanation of psychiatry and psychoanalysis, but may develop out of a much more extensive process of group interaction. One psychiatrist, in discounting the individual or personal problems of the patient, has stated that "since he worked up his psychoses in the group, he can never be cured until he has worked out his recovery in a group."¹⁷ As a recent research state-

¹⁵ Bruno Solby, "Group Psychotherapy and the Psychodramatic Method," in *Group Psychotherapy, A Symposium*, New York: Beacon House, 1948, pp. 50-51.

¹⁶ See Hester B. Cruthers, *Foster Home Care for Mental Patients*, New York: The Commonwealth Fund, 1944. Also George Kent, "Family Care for the Mentally Ill," *Survey Graphic*, June, 1948.

¹⁷ Cody Marsh, "Group Treatment of the Psy-

ment on the relation of mental hygiene and socio-environmental factors indicated, "the possible existence of group character structures, the stresses put on man by changing conditions or by the excessive demands of the culture, the sources of and the effect of loneliness and social isolation, and the techniques and effects of social esteem and social punishment on personality, these and many other problems need careful and continued investigation."¹⁸

While sociologists have used the group approach in their research in criminology more than in any other field, the verification of the findings through experimental manipulation of the social world of offenders, using group methods, has not been extensively investigated. Here the problem is not so much group integration in the narrower sense, for that is usually present, but of group reintegration into the norms of the larger society. Individual clinical methods of treating potential or actual delinquency where, incidentally, a sociologist is rarely on the staff, have not demonstrated any marked success.¹⁹ Some have suggested that rehabilitative efforts in correctional institutions should be on a group basis, with constant observation of the inmates' informal group structure and with efforts made to redirect these natural groups, rather than individuals, into a rehabilitative program.²⁰ Similar group programs might be employed in probation and parole work. Unfortunately, there are few skilled persons available for this type of approach. Efforts have been made in some directions to work on problems of deviant attitudes on a group basis, incorporating, for example, an entire delinquent group within a conventional framework. In one example

chooses by the Psychological Equivalent of the Revival," *Mental Hygiene*, 15: 341, April, 1931.

¹⁸ R. H. Felix and R. V. Bowers, "Mental Hygiene and Socio-Environmental Factors," *The Milbank Memorial Fund Quarterly*, 26: 134, April, 1948.

¹⁹ See Sheldon and Eleanor Glueck, *Juvenile Delinquents Grown Up*, New York: Commonwealth Fund, 1940.

²⁰ *Bulletin Correctional Service Associates*, Volume I, No. 1, 1947.

of this type the California Youth Authority, in co-operation with the War Department, in 1944 placed two groups of about 150 seriously delinquent boys in army arsenals to work side by side with several thousand civilian men and women. The army furnished barracks and met the boys' needs. Efforts were made to restore the delinquents' confidence in themselves and to change their roles by incorporating them into the norms and objectives of conventional society. It was a group process enabling individuals to get rid of unacceptable behavior by participation. On the surface the program seems to have made marked changes in work habits, in the conceptions of themselves, and in changing anti-social group objectives which are reinforced in conventional institutional groups.²¹ Some of the recent changes in the Swedish correctional system which involve rather extensive social participation also present experimental leads.²² In several correctional institutions chapters of Alcoholics Anonymous have been established which may affect other attitudes of offenders through group interaction.

During the recent war the armed services, and lately various state correctional institutions, have introduced group psychotherapy into the treatment of offenders. In New Jersey where they have recognized that all prison inmates are by no means mentally abnormal this work is called "guided group interaction." Again, the success that has been encountered in efforts of this type may not be the result of psychiatric theory or of the therapist but of the group situation. It sets in operation group forces directed toward socially accepted goals partially to counteract the anti-social group conniving that goes on so extensively in correctional institutions. The limited literature indicates that these groups objectively examine their experiences and the reasons for their confinement rather than relying upon prison

rationalizations, that there is growth in the capacity of the individual and the group to adjust, and that frequently an *esprit de corps* develops, particularly in the realization that they are helping others. Even personality characteristics appear to be modified for "the belligerent, over-assertive, anti-social rehabilitee is brought into line by his fellows and the asocial, shy, withdrawn person is drawn into the conversation."²³

One of the most promising research leads in social disorganization has undoubtedly been the efforts to bring about local community reintegration through the group approach and citizen participation. Known as neighborhood councils or other names, they are becoming increasingly widespread and appear to have a sound theoretical basis, both in accomplishing group redefinitions of situations and in giving the individual a place of belonging in the local structure. Studies of the success and failures of such efforts at group organization, as well as their relation to rates of delinquency, crime, mental disease, suicide, race and ethnic conflict, and alcoholism should be of primary interest to the sociologist. Here he can make his contribution by studying the social processes involved and the group variables which must be taken into account in successfully controlling the situation from a scientific point of view. Studies of informal groups and of agencies of moral risk such as the tavern, the dance hall, the political club, might well go along with investigations of the local community program.²⁴ Since sociologists assume that the disintegration of the community and the lack of consensus result in the increase of various forms of disorganization, the reintegration of the groups in the community should reduce such disorganization. If the answer is negative, then the factors should be isolated and studied. It appears, for example, that in deal-

²¹ Described in John R. Ellington, *Protecting Our Children from Criminal Careers*, New York: Prentice-Hall, Inc., 1948, pp. 95-111.

²² Thorsten Sellin, "The Treatment of Offenders in Sweden," *Federal Probation*, 12: 14-18, June, 1948.

²³ Joseph Abrahams and Lloyd W. McCorkle, "Group Psychotherapy of Military Offenders," *American Journal of Sociology*, 51: 458, March, 1946.

²⁴ See William F. Whyte, *Street Corner Society*, Chicago: University of Chicago Press, 1943, for a model of this type of research.

ing with ethnic, racial and religious group tensions the small intimate groups are the source of the individual's definition and that such problems must be dealt with on a group basis. As Williams has pointed out, however, there is a need in this field for the "further development of methods and materials for direct study of group behavior."²⁵

Probably the most intriguing of all group approaches to social reintegration have been those where groups have informally assumed a major responsibility for dealing with a common problem, as those formed to aid in the rehabilitation of the tubercular, of youth councils to deal with delinquency and other adolescent difficulties, of those formed to overcome the loneliness of old age, and of groups for alcoholics and former mental patients. In each instance the group helps to integrate the individual, to overcome stigma, to change his conception of himself, and to make him feel again the solidarity of the group behind the individual. Without going into the question of whether the high claims of success for Alcoholics Anonymous are entirely justified, we are still in the dark as to exactly what effect takes place. After contrasting European and American group methods of working with alcoholics, Bales has indicated that A. A. involves a "network of personal relations that are made up of obligations, friendships, and other personal influences. The network insures that each person is a focal point of strong inter-personal relations."²⁶ There are indications that in these group processes there is established "we" feelings for the "I," the individual is given a feeling of group incorporation, and group oriented rather than individualistic and materialistic goals are furnished. Probably most important is the redefinition,

²⁵ Robin M. Williams, Jr., *The Reduction of Intergroup Tension: A Survey of Research on the Problems of Ethnic, Racial and Religious Group Relations*, New York: Social Science Research Council, 1947, No. 57, p. 134.

²⁶ Freed Bales, "Types of Social Structure as Factors in 'Cures' for Alcohol Addiction," *Applied Anthropology*, April-June, 1942, p. 8. Also see his "The Therapeutic Role of A. A. as Seen by the Sociologist," *Quarterly Journal of Studies in Alcohol*, 5: 267, 1944.

through group experience, that is made in social values including those related to alcoholic drinking. Possibly we have an indication of the essential etiology of such behavior as alcoholism in this group treatment in that we also see other factors involved besides mere childhood emotional insecurity, for the alcoholic appears to have increasingly pulled himself away from meaningful or conventional group situations. In fact, the so-called religious emphasis in A. A. may be explained in terms of Durkheim's thesis that religion represents essentially the group and the feeling of getting outside of one's self by identification with others.²⁷ Such organizations represent, in part, the restoration of many of the characteristics of a folk society in the modern urban world.

Experiments with group integration have been developed generally without the aid of sociologists. At the risk of possibly destroying some of the mysteries which contribute therapeutically to their activities it is suggested that scientific analysis of these and similar activities may make possible more unique contributions by sociologists. Certainly it seems likely that psychiatrists and clinical psychologists are in no position to make as adequate studies of these or of the other types of group therapy approaches. Their background of theoretical training and point of emphasis have not been oriented in either the direction of social groups, with the possible exception of the family, or of the general society and its cultural value systems. Out of such study by sociologists could come not only additions to a body of theory but more adequate control of social and personal disorganization as well. Such knowledge as we could achieve about the group in relation to social disorganization could, in turn, be applied to social organization. Such emphasis on group situations would furnish not only a point of view for sociologists but also the necessary requirements of all science, the testing of knowledge in an attempt to control a problem situation.

²⁷ Emile Durkheim, *The Elementary Forms of Religious Life* (Swaine Trans.), 1912.

ESTIMATING THE NET EFFECT OF A COMMERCIAL MOTION PICTURE UPON THE TREND OF LOCAL PUBLIC OPINION*

J. E. HULETT, JR.†

University of Illinois

NOBODY doubts any longer that motion pictures affect the knowledge, opinions, and emotional states of the spectators. The effects are various, and in many cases they are unexpected; they depend upon the content of the film and the condition and predisposition of the spectators. Beginning with the Payne Fund studies and continuing to the present, a considerable body of research literature has grown up, based upon observation and measurement of changes in opinions and attitudes of persons exposed to films. These studies, however, have been almost entirely social psychological studies of individual cases or of individuals selected and grouped arbitrarily (instead of "naturally")—studies of the social and other factors involved in the reactions of individual spectators of films.¹

Studies of this kind—especially those by Bruner and Fowler and Wiese and Cole—represent stimulating approaches to the problem of what is the contribution of the movie experience to the attitudinal structure of the individual. But there is no way of directly making estimates or predictions from such

studies which will answer the question about motion pictures which is the primary concern of the sociologist and the student of public opinion: granted these effects of motion pictures upon individual spectators, how can we discover the net effect of the motion picture upon the public opinion process and upon collective action in the whole community? This problem, first conceptualized by Cressey² and later discussed by Stouffer³ in his paper on the design of experiment in communications research, is now being attacked by the students of the influence of the radio and other media, and considerable progress has been made. But many students of the movies continue to conceptualize their problem in such a way that the research always tells us something about the individual spectator and nothing, or almost nothing, about the effects on community opinions and collective action that may have occurred as a result of the commercial showing of a film. The present study reports an attempt to devise a method of attacking this latter problem.

THE PROBLEM

It may be taken for granted that no commercially exhibited film is ever seen by everybody in a community, nor is it ever seen by a representative sample of the community. Those who do see it (a group composed of motion picture addicts and others attracted by the star, the advertising, or

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¹ Solomon P. Rosenthal, "Change of Socio-Economic Attitudes under Radical Motion Picture Propaganda," *Archives of Psychology*, vol. 25, no. 166, 1934; Jerome Bruner and George Fowler, "The Strategy of Terror: Audience Response to *Blitzkrieg im Westen*," *Journal of Abnormal and Social Psychology*, vol. 36, pp. 561-574, 1941; Mildred J. Wiese and Stewart G. Cole, "A Study of Children's Attitudes and the Influence of a Commercial Motion Picture," *Journal of Psychology*, vol. 21, pp. 151-171, 1946; Louis E. Raths and Frank N. Trager, "Public Opinion and *Crossfire*," *Journal of Educational Sociology*, vol. 21, pp. 345-368, 1948.

² Paul G. Cressey, "The Motion Picture Experience as Modified by Social Background and Personality," *American Sociological Review*, vol. 3, pp. 516-525, 1938.

³ Samuel A. Stouffer, "A Sociologist Looks at Communications Research," in Douglas Waples (ed.), *Print, Radio, and Film in a Democracy*, Chicago 1942, pp. 133-146.

perhaps the "message" if there is one) may be influenced by the film, however, and some of the audience will communicate portions of this influence to interested acquaintances through discussion of the film. Collective action on some relevant issue or, in the absence of an issue, inactive public opinion in the community will involve the attitudes and opinions of more than this "exposed" group, however; it will also involve many of the larger number of community members who were unexposed or not so directly exposed to the influence of the film. After the showing of a film, therefore, local public opinion on the issues with which the film is concerned will have been influenced directly or indirectly by the film itself (among the portion of the population which saw or discussed it) and by the multitude of past and present influences other than the film itself which play upon the various segments of the population and which cause the trend, or "drift," of opinion which takes place apparently independent of the film. In order to estimate the net influence of the film it is necessary to measure (1) the changes in opinion occurring in the portion of the population exposed to the film and (2) the changes in opinion taking place in the unexposed portion of the population; further, (3) it is necessary to improvise some way of estimating what would have been the trend of opinion in the exposed portion of the population if the film had not been shown. This latter step is required because there is no certainty that the trend or drift of opinion in the exposed group would have been the same as the trend in the portion which did not choose to expose themselves to the film.

In brief, the commercial movie audience is composed of, so to speak, a "self-selected" but continually shifting segment of the total community population,⁴ whereas participation in the public opinion process and decision on local issues involve a much larger

portion of the total. The immediate problem here is to observe and record changes in the opinions of both the exposed and unexposed portions of the population which occurred during an interval of time which included the commercial showing of a motion picture in the community, and from these data to estimate the net changes attributable to the film that may have occurred in the opinions (verbal responses) of a representative sample of the community on issues connected with the topics of the film.⁵

THE PROCEDURE

The panel technique for the study of change in opinions was used in the study. The panel consisted originally of a stratified sample of about one per cent of the estimated 30,000 persons aged 20 and over living in the local community (Champaign-Urbana, Illinois) as more or less permanent residents. The sample was drawn proportionately from the several socio-economic, racial, sex, and age strata of the population by means of the quota control method, the economic strata being determined by a classification of the various residential areas into four socio-economic levels. Randomness within the various residential areas was assured by an area sampling method whereby the interviewers were instructed in filling their quotas to obtain interviews only in specifically designated city blocks which had been selected at random from a city map.

The members of the panel were inter-

⁴ Paul F. Lazarsfeld, "The Effects of Radio on Public Opinion," p. 68, and Samuel A. Stouffer, *loc. cit.*, p. 142, in Douglas Waples, *op. cit.*

⁵ It is recognized that to formulate in these terms the problem of studying the complex propaganda effects of a motion picture is virtually to ignore the specific details and patterns of content of the film which (instead of the entire film) may be the immediate causes of the changes of opinion. However, this small preliminary study was not geared to the larger task of discovering *what elements* of the film produced the results, although some attention to items of content is given in a few scattered references in the analysis. In view of the current preoccupation of some researchers with content analysis of films, it might be mentioned that a motion picture has an impact on the spectators as a *totality* in addition to the several items of content, and this effect of the totality should not be minimized or neglected.

viewed twice. The first interviews were conducted during a period several days in length shortly before the film was to be shown locally and before any local advertising for the film had appeared. The second interviews occurred during a similar period beginning one week after the film was shown. Both interviews were based on questionnaires. The questionnaire used for the second interviews repeated most of the items of the first questionnaire and contained a number of additional items asking whether the respondent had seen the film or discussed it with anyone, and what were his specific responses, if any, to the film.

Despite all precautions, the panel suffered a "mortality" of about 30 per cent during the period between the interviews; this mortality resulted in a reduction of the panel membership from 298 at the first interviews to 207 at the second. Fortunately, the losses seem not to have markedly disturbed the geographical distribution or the social composition of the sample except in the sex ratio: about two-thirds of the losses were male. A number of detailed checks were made (on the basis of answers given during the first interviews) in order to discover whether the withdrawal of the individuals who dropped out of the panel produced any marked bias in the opinion data yielded by the remainder of the panel. These checks revealed little to suggest that this group possessed any single common attitude-structure with respect to the topics and issues involved in the study or that they were significantly different in this regard from those remaining. The principal statistical consequence of the panel mortality was to reduce the size of the sample which was available for the analysis and to increase the magnitude of the standard errors of the differences between the various percentages. This matter therefore is not considered to be a fatal defect of the study although it does diminish the reliability of the results.

The "Exposed" Group: At the beginning of the project it was hoped that the sample population could be divided into three categories: (1) those who saw the film, (2)

those who did not see the film but who reported discussing it with someone, and (3) those who reported that they had neither seen nor discussed the film, the purpose of this division being to get at three different levels of participation in the movie experience. However, so few respondents saw the film (only 25 out of 207, or 12 per cent) that the idea of three groups had to be abandoned—statistical analysis of a sample category containing only 25 cases would have been unprofitable in the present instance. The best that could be done was to combine the members of the first two categories, the spectators and the discussants, into a single group (54 individuals, or 26 per cent) which yielded a total that is somewhat more satisfactory. This combined group (hereafter called the exposed group) turned out to be different, in some cases significantly different, from the members of the original third category (hereafter called the unexposed group), as might have been expected.⁶ In addition to certain differences in social and economic characteristics (Table I), the initial opinions of this group tended to be more favorable to the point of view of the film on several questions (see below).

Derivation of the Measure of Change: In the present study we are principally interested in the effect of the film on the distribution of opinions in the entire sample. If the opinions of the unexposed portion of the population remained stable during the

* Though this step does not yield as detailed a breakdown of the data as was originally hoped for, it does yield a separate class of respondents who reported that they had been stimulated by the film, either through direct exposure or by interpersonal contact. These individuals are thus differentiated from the remainder of the sample population and can be treated as a distinct category. Ideally, of course, we do not actually have here two completely discrete groups, the one having been stimulated by the film and events connected with it, and the other unstimulated. During the interval between interviews, persons included in our so-called "unexposed" group may have been influenced by discussions which they could not recall, by the recall of ideas and information previously acquired from reading or the radio and stimulated by the local film publicity, by our interviews, and so on.

interval between interviews, an estimate of the change in the total sample due to the movie could be made by simply attributing any change to this cause. However, opinions in the unexposed group did not remain stable; they seemed to "drift" even though the members of this group reported they had neither seen nor discussed the film. At first glance, this might appear to be no obstacle, because one could estimate the influence of

experience with polio (Table I), and being the parents of young children. The obvious need is for an unexposed control group matched with the exposed group according to these significant characteristics.

A synthetic control group of unexposed cases was constructed by choosing the schedules of 60 individuals who did not view or discuss the film, but who had approximately the same social and economic characteristics

TABLE I. COMPARISON OF PERCENTAGE DISTRIBUTION OF EXPOSED GROUP (54 CASES) WITH UNEXPOSED GROUP (153 CASES) IN TERMS OF SEVERAL INDICES, AND LEVEL OF SIGNIFICANCE OF DIFFERENCES BETWEEN THE DISTRIBUTIONS¹

	Ex- posed	Unex- posed	Ex- posed	Unex- posed
<i>Socio-economic group:</i>				
"A"	13.0	11.8		
"B"	37.0	24.8		
"C"	33.3	48.4		
"D"	16.7	15.0		
(P = .23)				
<i>Education:</i>				
Completed College	22.2	13.1		
Completed High School	51.9	43.1		
Completed Grade School	20.4	33.3		
Some Grammar School	3.7	9.8		
No Schooling	1.8	0.7		
(P = .05)				
<i>Frequency of attendance at movies:</i>				
Rarely or never	20.4	32.7		
Every 2-3 months	9.2	13.7		
1-3 times monthly	31.5	30.7		
Once a week or more	38.9	22.9		
(P = .09)				
<i>Age:</i>				
20-39			61.1	43.8
40-59			29.6	36.6
60 and over			9.3	19.6
(P = .05)				
<i>Sex:</i>				
Male			33.3	44.4
Female			66.7	55.6
(P = .16)				
<i>Marital status:</i>				
Married			87.0	93.5
Single			13.0	6.5
(P = .14)				
<i>Child or close relative had polio?</i>				
No			79.6	91.5
Yes			20.4	8.5
(P = .03)				

¹ Indicated by the value of P corresponding to the Chi-square computed for each pair of distributions.

the movie by subtracting the drift from the total change. However, this procedure would involve an unwarranted assumption: that the rate of drift would have been the same in both the exposed and the unexposed groups, if the movie had not been shown. These groups are in fact quite different in several characteristics which logically would have some effect on the formation of attitudes about the treatment of polio (the main issue in the film): education, age,

as the actual exposed group, and the trend, or drift, of opinions in this selected group was calculated. The rates of change thus ascertained were then applied to the initial scores of those individuals who did see or discuss the film, thus providing estimates of what the opinions of the exposed group would have been had they not been exposed. These estimates were then combined with the "after" returns of the unexposed group. Thus a set of hypothetical or assumed final per-

centages was constructed which could be thought of as the distribution of opinions that would have existed in the entire sample at the time of the second interviews if the movie had not been shown. This procedure permits the use of two sets of opinion data: (1) "before" and "after" opinions as actually discovered through the interviews, and (2) a set of hypothetical or assumed "after" opinions constructed on the assumption that if the film had not been shown, the opinions of those persons who would have made up the group of movie spectators and discussants would have "drifted" between interview periods at the same rate and in the same direction as the opinions of a group with similar social and economic characteristics.⁷ The measure of change employed in the analysis below is simply the difference between the change in opinions actually recorded in the interviews taken before and after the film was shown, and the change in opinions that would have occurred as a consequence of the "drift" if the movie spectators and discussants had not been exposed to the film, this latter change being calculated on the basis of a synthetic population composed of the actual unexposed group and a hypothetical exposed group roughly similar to the actual exposed group except for exposure to the film.

THE FILM USED IN THIS STUDY—
"SISTER KENNY"

Comparatively few pure propaganda films are commercially shown in the United States. However, a number of "entertainment-propaganda" films have appeared in recent years—films like *The Best Years of Our Lives*, *The Farmer's Daughter*, and others—which carry a message though their principal appeal is to the general entertainment-seeking public. One of these films, *Sister Kenny*, was

⁷This assumption, of course, is not without hazards. For example, while the synthetic "control" group was matched with the "experimental" group for social and economic characteristics, it was impossible in this small study to match the groups also for common initial attitude-structures. To attain this more ideal condition would have required several times as many schedules as were actually available to choose from.

selected as the basis for the present study and preparations were begun several months in advance of the date upon which the film was to be shown in a first-run movie house in Champaign-Urbana, Illinois.⁸

The film (starring Rosalind Russell and Alexander Knox) portrays a romanticized account of Sister Kenny's life, and presents a one-sided version of the central issue: a clear and dramatic contrast is drawn between the mild physical therapy for infantile paralysis discovered and advocated by Kenny and the grim and severe therapy advocated by "orthodox" practitioners as represented in the film. The advance publicity of the film contained the usual phrases and formulas characteristic of movie advertising—examples are: "The wedding gown that waited," "The picture with an amazing soul," "This woman was made for more than romance," "The screen's warmest chapter of human experience," "She won fame . . . but lost love," and so on.⁹ National publicity for the film was provided in articles published in several mass circulation magazines, including *The American Weekly* (September 29, 1946) and *Life* (September 16, 1946), and in advertising space in newspapers and magazines bought by the producers. Without any unusual fanfare, the film had a short (four day) run in Champaign-Urbana in the Fall of 1946; it was seen

⁸The author's interest in the Kenny infantile paralysis healing movement dates from 1945, when he published an article on "The Kenny Healing Cult: Preliminary Analysis of Leadership and Patterns of Interaction," *American Sociological Review*, vol. 10, pp. 364-372. The study of the public opinion phase of the film on Sister Kenny's life stems from suggestions made in this original article as to the role of the public in what is essentially a medical and orthopedic issue.

⁹The advertising generally soft-pedaled the controversial nature of the film; of 20 advance proofs of the newspaper cuts prepared for use by theater managers in local advertising, only three referred to the fact that there was some opposition to the Kenny method. The exhibitors' handbook of the film contained copy for several "canned" newspaper stories which specifically disclaimed bias in the handling of the story in the film. (Mr. S. Barret McCormick of RKO Radio Pictures, Inc., producers of the film, kindly provided the writer with a collection of this material.)

by about 8,000 adults, including an undetermined number of university students and people from outlying rural areas.

CENTRAL AND PERIPHERAL ISSUES
OF THE FILM

Echoes of the controversy over the Kenny theory and method have occasionally been heard by the general public, and two of the main issues in the controversy are specifically pointed up in the film for public evaluation: (1) whether this therapy is in fact more effective than any other method of treatment,¹⁰ and (2) whether the physicians and orthopedists have exhibited prejudice and undue conservatism with regard to this innovation offered by someone outside the medical profession. These two may be thought of as the central issues brought out by the film, and around which the spectators are asked to form opinions. The showing of the film presented an opportunity to investigate two other foci of opinion which are peripheral to the central issues. The first of these is the question whether members of the community feel that "the public" has a role to play in the settlement of the issue and, incidentally, whether the families of polio patients should urge their physicians

¹⁰ From the standpoint of modern conventional medical practice, this apparently is no longer regarded as an issue, inasmuch as it can hardly be said that there now exist two distinct systems of treatment, the Kenny and the "orthodox" systems. The most valuable elements of the procedures advocated by Miss Kenny have been gradually incorporated into standard practice during the last few years. With considerable credit to the energy, persistence, and power of persuasion of Miss Kenny, this development actually represents something of a medical revolution in the diagnosis and (for the most common types of cases) therapy of infantile paralysis. However, the Kenny partisans remain dissatisfied with this only partial adoption and the modifications of the Kenny system now in common use, and have set up and maintain their own clinics and fund-raising organization. So far, they have been unable to resolve their disagreements with such groups as the National Foundation for Infantile Paralysis which approach the problem of polio in a more eclectic spirit. From the standpoint of the Kenny group, therefore, there seems still to be an issue.

to apply the Kenny therapy to the patients. Another issue peripheral to the main point of the film arises in connection with the use of money voluntarily contributed by the public for combating polio. The rivalry between the Kenny organization and the National Foundation for funds may on occasion give rise to an issue in the community or require a decision in individuals approached by representatives of both organizations.¹¹ Changes in opinions on these four issues will now be considered in order, with a view to discovering what effects, if any, were related to the showing of the film.¹²

(1) *Opinions regarding the effectiveness of the Kenny method in comparison with other methods:* When the film was first released, fears were expressed that the audiences would be induced to believe all its implications to the effect that Kenny-treated patients recover completely and rapidly, whereas most patients subjected to "orthodox" treatment will always be crippled.¹³ Much was made in the movie of the crippling that remained in patients treated by other than Kenny methods; there was one dramatic scene in which a sound and healthy Kenny-treated youngster was directly and pointedly contrasted with an unhappy, crippled boy in braces who was supposed to represent the results of orthodox treatment.¹⁴

The schedule contained two direct questions concerning the respondents' opinion of the general efficacy of the Kenny treatment in comparison with other treatments. Replies

¹¹ An issue over the expenditure of tax revenues arose during the Summer of 1948 in Centralia, Illinois, where there is a Kenny clinic. State support for this clinic was withdrawn over a question of medical supervision of the treatment and considerable public discussion was precipitated by the incident. The issue finally was settled by compromise.

¹² The order in which the schedule items are discussed in this report is not the order in which they appeared on the questionnaire.

¹³ *Life, op. cit.*, p. 77.

¹⁴ This particular scene apparently was less effective than might have been expected. Only one out of the 25 persons of our sample who saw the movie mentioned it in response to the question: "What scene or scenes in the movie were most impressive to you?"

to these questions before and after the film revealed no significant¹⁵ changes of opinion in the community that could be attributed to the film (one of these questions is reproduced in Table II-A). Two additional questions were designed to discover opinions about the patients' chances of escaping crippling with the Kenny treatment. One asked whether the patient had about the same chance of crippling with the Kenny treatment

as with other treatments¹⁶ (Table II-B). On this item the trend of opinion in the population who did not see the film was unfavorable to Kenny, as it apparently would also have been in the exposed group if they had not seen the film. Because they saw the film, however, their opinions became more favorable to Kenny on this point, producing a small net increase of about five per cent in the favorable replies among the whole

TABLE II. OPINIONS ON THE COMPARATIVE EFFICACY OF THE KENNY TREATMENT BEFORE AND AFTER THE MOVIE—REPLIES OF EXPOSED AND UNEXPOSED GROUPS AND THE TOTAL (THREE QUESTIONS)
(Percentage Distributions)

	Exposed		Unexposed		Total			% Change
					Observed		Assumed ¹	
	Before	After	Before	After	Before	After	After	
	(n = 54)		(n = 143) ²		(n = 197)			
A. Question: How good, that is, how effective, do you think the Kenny treatment for infantile paralysis is, in comparison with other modern, accepted methods?								
No opinion	18.5	13.0	28.0	26.6	25.4	22.8	23.9	-1.1
Not as good	1.9	1.9	1.4	1.4	1.5	1.5	1.5	0.0
As good but no better	37.0	40.7	33.6	35.0	34.5	36.6	38.1	-1.5
Better	42.6	44.4	37.0	37.0	38.6	39.1	36.5	+2.6
B. Question: Some people say that a person has about the same chance of being permanently crippled from polio with the Kenny treatment as with any other modern, accepted treatment. Do you agree, or disagree?								
Undecided	22.2	20.4	29.4	32.2	27.4	28.9	28.4	+0.5
Disagree	38.9	48.1	40.5	31.5	40.1	36.1	31.0	+5.1
Agree	38.9	31.5	30.1	36.3	32.5	35.0	40.6	-5.6
C. Question: Do you think that if a person has a severe case of polio, only the genuine Kenny treatment can save him from being permanently crippled?								
No opinion	18.5 ³	18.5	33.6 ³	28.0	29.4	25.4	23.3	+2.1
Yes	25.9	22.2	15.4	11.9	18.3	14.7	12.7	+2.0
No	55.6	59.3	51.0	60.1	52.3	59.9	64.0	-4.1

¹ See text for method of derivation.

² Ten of the unexposed group who had never heard of Sister Kenny were omitted from the tabulation.

³ The difference between these two distributions is fairly reliable; the value of P corresponding to the Chi-square calculated from them is .06.

¹⁵ Computations of the significance of differences between whole distributions by means of the Chi-square test and between single proportions by means of Critical Ratios indicate that with such small numbers of cases the differences would have to be on the order of about nine per cent for the entire sample and nearly 20 per cent for the exposed group before they could be regarded as statistically significant.

¹⁶ This item (Table II-B) appears to be ambiguous, because a "Disagree" reply might be taken to mean that the respondent felt either that a patient has a better chance, or that he has a worse chance, with the Kenny treatment. The ambiguity is more apparent than real, however; practically all respondents who replied "Disagree" meant that they thought a patient had a *better* chance with Kenny.

TABLE III. OPINIONS ON CONSERVATISM OF THE MEDICAL PROFESSION BEFORE AND AFTER THE MOVIE—PERCENTAGE DISTRIBUTIONS OF REPLIES OF EXPOSED AND UNEXPOSED GROUPS AND THE TOTAL (THREE QUESTIONS)

	Total							
	Exposed		Unexposed		Observed		Assumed ¹	
	Before	After	Before	After	Before	After	After	
	(n=54)		(n=153)		(n=207)			% Change
A. Question: Do you think that patients ever receive out-of-date treatments because the doctors are slow to adopt new methods?								
No opinion	1.9	3.7	5.8	7.8	4.8	6.8	6.3	+0.5
Yes	87.0	85.2	75.2	70.6	78.3	74.4	74.4	0.0
No	11.1	11.1	19.0	21.6	16.9	18.8	19.3	-0.5
B. Question: Do you think that the public is ever denied the benefit of a good new method of treatment because a few prominent medical leaders have a personal prejudice against it?								
No opinion	9.3	1.9	10.5	11.1	10.1	8.7	9.2	-0.5
Yes	50.0	61.1	41.8	46.4	44.0	50.2	48.8	+1.4
No	40.7	37.0	47.7	42.5	45.9	41.1	42.0	-0.9
C. Question: Most doctors and hospitals nowadays approve a part of Sister Kenny's theories and treatment for polio, but not all of them. Do you think that this is because they have not all been proven by scientific research, or is there some other reason? ²								
No opinion	*	18.5	20.4	21.7	19.6	20.8	19.8	16.2
Not all proved by scientific research	46.3 ³	63.0 ³	52.4	58.0	50.8	59.4	57.9	+1.5
Some other reason	35.2 ⁴	16.6 ⁴	25.9	22.4	28.4	20.8	25.9	-5.1

¹ See text for method of derivation.

² The n of the unexposed group for this item is reduced by ten persons who never heard of Sister Kenny; the n of the total therefore is also smaller by ten.

³ The difference between these two percentages is only 1.8 times its standard error.

⁴ The difference between these two percentages is 2.3 times its standard error.

panel. The other question (Table II-C) asked if only the Kenny treatment could save a patient from permanent crippling, thus presenting the respondent with a clear choice between the Kenny treatment and the others. Here, the trend in the entire sample appears to have been unfavorable to this view but the shift in the exposed group was less; the effect of the presence of the exposed group in the total was only to "retard" the unfavorable trend.¹⁷

¹⁷ If the changes in the exposed group alone are examined (Table II), it becomes noteworthy that there were no large and consistent shifts of opinion on this, one of the central topics of the film, even among those exposed to it. This reflects the already well-known inability of a single exposure to propaganda to produce much change of opinions. It also

(2) *Opinions regarding the conservatism and clannishness of physicians:* Advance publicity for the film described it as a drama of struggle and conflict. The film itself revealed the forces against which the heroine was struggling—not polio quite so much as

reflects the seldom-mentioned but important cynicism of community members about the realism and accuracy of films. One of our questions asked: "Do you think that movies about the lives of famous people stick closely to the true facts, as a general rule?" Approximately one-half of the respondents answering this question replied "No." Only about ten per cent gave no answer. A survey of respondents' comments revealed the prevalence of the belief that this supposed lack of realism and accuracy was due to a tendency of "Hollywood" to glamourize the characters and stories in order to enhance box-office appeal.

the conservative and cliquish medical men. One physician in the film was portrayed as sympathetic, but his support of Kenny was rather bumbling and ineffectual, however warmhearted it may have been. While the film presented the most intense phase of the struggle against medical tradition as having occurred in Australia two decades ago, it also indicated that the attitude of American physicians, though respectful, was skeptical and in one instance sharply critical. The audience was not informed that the medical world has yielded somewhat and has adopted many of the elements of Kenny therapy into standard practice since 1941; instead, the film ended on a note of "the fight must go on." Some of the questionnaire items were designed to elicit opinions about (a) conservatism and (b) clannishness in the medical world.

(a) Both before and after the film, about three-fourths of the members of the panel replied "Yes" to a general question asking whether patients ever receive out-of-date treatments because doctors are slow to adopt new methods. First and last, the exposed group was more agreeable to this view than the unexposed group, but the film experience appears not to have altered the trend of opinion (Table III-A). "Doctor Brack," the conservative medical leader and Sister Kenny's principal opponent in the film, was portrayed as the personification of medical prejudice and disdain. In response to this characterization there was a slight increase among the exposed group in the percentage of "Yes" replies to a question (Table III-B) asking whether the personal prejudice of a few prominent medical leaders ever prevents the public from getting the benefit of new methods of treatment, but this increase was reflected hardly at all in the total sample.¹⁸ One question was designed to

discover within very broad limits what the respondents thought were the motives behind the physicians' hesitancy in accepting the Kenny ideas—was it because of "scientific" or "some other" reasons? There was a shift of opinions, unfavorable to the Kenny argument, of about 18 per cent among the exposed group, which was only slightly reflected in the index of net shift for the total group (Table III-C).¹⁹

(b) After the film was shown, 68 per cent of the total sample agreed that a person outside the medical profession may have a difficult time getting the doctors to "try out" a new method of treatment. This represents an increase of about 13 per cent, a third of which apparently was due to the effects of the film on the exposed group, who moved further in the favorable direction than they would have been expected to move if they had not been exposed to the film (Table IV-A). It cannot be said that this is a reliable shift, however, because of the small numbers involved. On a question asking whether the respondents thought the doctors *oppose* a new treatment because it was developed by an outsider, the shift of opinion was in the same direction but smaller (Table IV-B).

The film strongly implied that the Kenny method did not receive a fair trial at the hands of the doctors, and this implication was sympathetically received by the exposed group, resulting in an increase in the "No" replies to the schedule question on this point (Table IV-C). Judging by the behavior of our synthetic "control" group, the exposed

film by the scene showing the caustic verbal exchange between Kenny and "Brack" in the hospital amphitheatre in the shocked presence of numerous nurses and medical students, and this scene was frequently mentioned by the spectators as the most impressive.

¹⁸ This question unfortunately contained a popular cliché—"proof by scientific research"—which in itself probably elicits a favorable response from the community members. This factor may have "loaded" the replies to the question. The shift in the unexpected direction by the exposed group may also have been related to the cynical attitude toward the movies already mentioned.

¹⁸ It is interesting to note, from the spectators' comments on the film, that many of them seem to have come away more impressed by Miss Kenny's brash opposition to established authority as personified by "Dr. Brack" than by the contrast between her and the "orthodox" therapeutic results. This opposition was represented most dramatically in the

group should have moved in the opposite direction on this point; therefore, a net change of about seven percentage points favorable to the argument of the film was recorded for the entire panel.

(3) *Opinions regarding the role of "The Public" and of patients' families:* The role of "the public" in a medical controversy is

the public in the controversy.²¹ It is clear from our data, however, that only a small fraction of the members of the community were persuaded by the film that the public should take an active role in this particular controversy (Table V-A).

A question as to whether the public should intervene evokes a highly general attitude in

TABLE IV. OPINIONS ON "CLANNISHNESS" OF THE MEDICAL PROFESSION BEFORE AND AFTER THE MOVIE—PERCENTAGE DISTRIBUTIONS OF REPLIES OF EXPOSED AND UNEXPOSED GROUPS AND THE TOTAL (THREE QUESTIONS)

	Exposed		Unexposed		Total			% Change
					Observed		Assumed ¹	
	Before	After	Before	After	Before	After	After	
	(n = 54)		(n = 153)		(n = 207)			
A. Question: Some people say that unless a person is a doctor himself, he may have a hard time getting the medical profession to try out a new method he developed for the treatment of a serious disease. Do you agree, or disagree with this?	No opinion	3.7	5.6	9.8	5.2	8.2	5.3	+4.3
	Agree	63.0	79.6	52.3	64.1	55.1	68.1	+3.8
	Disagree	33.3	14.8	37.9	30.7	36.7	26.6	-31.4
B. Question: Do you think that the medical doctors oppose the use of a new treatment for disease because it was developed by a person outside the medical profession?	No opinion	7.4	7.4	10.5	4.6	9.6	5.3	+4.4
	Yes	25.9 ²	44.4 ²	34.6	43.8	32.4	44.0	+2.0
	No	66.7	48.2	54.9	51.6	58.0	50.7	-2.9
C. Question: Do you think that the Kenny treatment for infantile paralysis has been given a fair trial by the medical doctors in the U. S. ²³	No opinion	18.5	18.5	29.4	37.0	26.4	32.0	+0.5
	No	37.0	53.7	38.4	36.4	38.1	41.1	+7.1
	Yes	44.5	27.8	32.2	26.6	35.5	26.9	-7.6

¹ See text for method of derivation.

² The difference between these before-and-after percentages is significant, the value of the critical ratio being 2.05.

³ The n for the unexposed group on this question was reduced by ten cases who had never heard of Sister Kenny; the n of the total therefore is also smaller by ten.

not always clearcut, though patients, donors, and others do have the ability to put pressure upon the doctors in various ways.²⁰ Consequently, the partisans of an unorthodox system are occasionally tempted to "take their fight to the public" when medical acceptance comes slowly or not at all. In its propaganda aspect, the film *Sister Kenny* was an effort to increase the participation of

the respondent. Two other questions in the schedule offered the respondent an opportunity to express himself on a more specific matter: should the patient's family intervene in the treatment prescribed by the family physician? It was thought that if the film

²¹ This move was not entirely ill-advised. On one of our questions something over 60 per cent of the sample agreed to the general proposition that "a good many useful ideas have to be forced on the doctors by the pressure of public opinion."

²² J. E. Hulett, Jr., *op. cit.*, pp. 369-371.

were really convincing, its influence should be manifested in an increase in the "Yes" replies to questions of this kind. The film, however, had no such effect; on both questions built around this idea of direct family intervention the percentage distribution of

(4) *Opinions regarding the use of money voluntarily contributed to anti-polio organizations:* Most of the money expended for research on polio and treatment of victims comes from public voluntary subscriptions through two organizations: the National

TABLE V. OPINIONS ON THE ROLE OF THE PUBLIC IN THE KENNY CONTROVERSY AND ON THE ROLE OF PATIENTS' FAMILIES IN SELECTION OF THERAPY BEFORE AND AFTER THE MOVIE—
PERCENTAGE DISTRIBUTIONS OF REPLIES OF EXPOSED AND UNEXPOSED
GROUPS AND THE TOTAL (THREE QUESTIONS)

	Exposed		Unexposed		Total				% Change	
					Observed		Assumed ¹			
	Before	After	Before	After	Before	After	After	After		
	(n = 54)		(n = 143)		(n = 197)					
A. Question: Sister Kenny and the doctors have had a controversy over the merits of her theories and treatment for polio. Do you think there is any reason for the general public to take sides in this controversy, or should it be left up to Sister Kenny and the doctors?	No opinion	3.7	7.4	2.8	6.3	3.1	6.6	6.1	+0.5	
	Public should take sides	38.9	48.2	33.6	32.2	35.0	36.5	34.0	+2.5	
	Public should not take sides	57.4	44.4	63.6	61.5	61.9	56.9	59.9	-3.0	
B. Question: Suppose that the family of a polio patient find out that the doctor is not using the Kenny treatment. Do you think that this family should get another doctor in the case?	No opinion	14.8 ²	12.9	15.4 ²	11.2	15.2	11.7	10.2	+1.5	
	Yes	66.7	59.3	41.3	41.3	48.2	46.2	49.2	-3.0	
	No	18.5	27.8	43.3	47.5	36.6	42.1	40.6	+1.5	
C. Question: If your child had polio, do you think that you would leave the question of treatment entirely up to your regular doctor, or would you take special pains to see that the Kenny treatment was used?	No opinion	11.1 ²	7.4	11.9 ²	9.1	11.7	8.6	9.6	-1.0	
	Would see that Kenny treatment was used	61.1	57.4	38.5	37.1	44.7	42.7	42.2	+0.5	
	Leave up to doctor	27.8	35.2	49.6	53.8	43.6	48.7	48.2	+0.5	

¹ See text for method of derivation.

² The Chi-square comparing these two distributions is significant at the .003 level.

³ The Chi-square comparing these two distributions is significant at the .01 level.

replies shifted slightly in a direction opposed to the point of view of the film (Table V-B and C), and most of this change occurred in the group exposed to the film.²²

²² In this instance, as well as in others, a comparison of the before and after views of the exposed and unexposed groups gives support to the well-known fact that persons tend to expose themselves to propaganda whose views they already agree with. Even before the film was shown, the persons who were going to expose themselves directly or indirectly to its influence were clearly more likely than the unexposed group to agree that the family should

Foundation for Infantile Paralysis and the Sister Kenny Foundation. Publicity about the sharp disagreements between Sister Kenny and the National Foundation and the explanations issued by this organization²³ has never appeared in Champaign-Urbana.

intervene in the treatment to make sure that the doctor prescribed the Kenny method. Of the Chi-squares comparing the distributions on the two questions referred to in the text, one was significant at the .003 level and the other at the .01 level.

²³ J. E. Hulett, Jr., *ibid.*, pp. 369-370.

Consequently, the members of this community have never been faced by an issue based on this dispute. But the competition of the two organizations for contributions does exist locally. Because of this local competition, three items were included in the schedule to discover the effect of the film on the opinions of the panel about this division of effort in the fight against polio.²⁴ An almost negligible proportion (less than ten per

acteristic of both exposed and unexposed groups, the shift in the exposed group was greater. The reasons for this are ambiguous: some respondents expressed irritation at being solicited by an increasing number of organizations; some felt that "a concentration of effort" would be better; others believed that there was already too much conflict between competing organizations—they "should work together."²⁵

TABLE VI. OPINIONS ON THE DESIRABILITY OF MORE THAN ONE ORGANIZATION TO COLLECT AND EXPEND FUNDS IN FIGHTING POLIO—PERCENTAGE DISTRIBUTIONS OF REPLIES OF EXPOSED AND UNEXPOSED GROUPS AND THE TOTAL

	Exposed		Unexposed		Total			% Change
	Before	After	Before	After	Observed		Assumed ¹	
					(n = 54)	(n = 143)	(n = 197)	
No opinion	5.6	20.4	9.1	11.9	8.1	14.2	10.7	+3.5
One	37.0	38.9	30.1	35.0	32.0	36.0	37.0	-1.0
Two	57.4	40.7	60.8	53.1	59.9	49.8	52.3	-2.5

¹ See text for method of derivation.

cent of the panel) replied "Yes" to a question asking whether the respondent preferred to have his own contribution spent on the Kenny method only; the film did not affect this percentage. The respondents were also asked whether they thought there was a need for two organizations to combat polio (Table VI). The distribution of replies tended to shift away from the "Two organizations" position during the interval between the two surveys. While this shift was char-

²⁴ One of these items was put in because of remarks attributed to Sister Kenny in 1944 to the effect that she had never been adequately supported by the National Foundation (*ibid.*, p. 370). The item read: "Do you think that the National Foundation gives as much financial support as it should to the study and promotion of the Kenny treatment?" About three-fifths of the respondents had no opinion, and the remainder were evenly divided between "Yes" and "No." The movie had no significant effect on this distribution of replies.

SUMMARY

By means of a modified panel technique, an attempt has been made to estimate the net effect of a commercially exhibited entertainment-propaganda motion picture upon the trend of local public opinion. The trend over a period of time (within which the film was exhibited at a local theater) was measured by means of a questionnaire, and a method was devised for constructing a set of hypothetical opinion data intended to show the trend that would have occurred during the period if the film had not been shown. By comparing the observed trend (which includes the influence of the film) with the assumed trend (which supposedly omits the influence of the film), the direc-

²⁵ Only one respondent revealed a partisan political prejudice in his explanation. His bizarre reply was: "Stick with Sister Kenny—so long as the Roosevelts are with the National Foundation."

tion and magnitude of net changes are revealed.

Results from the study were generally negative—that is, the film *Sister Kenny* was an unexpectedly ineffectual propaganda instrument; first, because it appealed to only a very small group in the local community; second, because its arguments, though dramatically and skilfully presented, in most cases apparently were not intrinsically convincing;²⁶ and third, because most members of the community do not attribute accuracy and seriousness of purpose to films shown commercially. There seems to exist among the local audience a kind of "propaganda sophistication,"²⁷ and an expectation only of

²⁶ Still another characteristic of the film—that of placing the most intense phase of Kenny's struggle in a distant continent and in the past—may have further reduced its propaganda appeal. It is possible that some spectators got the impression that they were only looking at another of Hollywood's portrayals of settled phases of medical history, rather than at a description of a presently lively issue.

²⁷ The comments about the film made by approximately three-fifths of our small group of spec-

entertainment from commercial motion pictures, both of which militate against the effectiveness of a "one-shot" exposure to propaganda such as this film provided.

This report has dealt with changes in opinions only. However, after more intensive analysis, we might be in a position to venture a guess at what difference the film might have made in the outcome of some collective action—say, a local referendum on a tax levy for supporting a Kenny clinic. For this purpose we should have to take account of the fact that there are three (and perhaps more) groups in the community which have participated in the movie experience at different levels of intensity. We should also have to arrive at some basis for making assumptions regarding the different levels of intensity at which the members of these groups might participate in the public opinion phase of the action and in the action itself.

tators indicated that they felt (in varying degrees) that the presentation was biased.

A NEW DIMENSION OF STATUS. III. DISCREPANCIES BETWEEN THE ST SCALE AND "OBJECTIVE" STATUS*

HARRISON G. GOUGH
University of Minnesota

IN THE first paper in this series the development of a new kind of scale for socio-economic status was described.¹ This scale, called St, consisted entirely of items drawn from a personality inventory,²

and contained no overt references to status whatsoever. The scale had a test-retest reliability of .87, and yielded validity coefficients of .68 in the original sample of 223 high school students, and of .50 and .52 in two subsequent samples of 263 and 261 cases, respectively. The Sims Score Cards,³ the American Home Scale,⁴ and the Home

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¹ Harrison G. Gough, "A New Dimension of Status. I. Development of a Personality Scale," *American Sociological Review*, XIII (Aug. 1948), 401-409.

² Starke R. Hathaway and J. Charnley McKinley, *The Minnesota Multiphasic Personality Inventory*, Minneapolis: University of Minnesota Press, 1943.

³ Verner M. Sims, *Sims Score Card for Socio-Economic Status*, Bloomington, Illinois: Public School Publishing Company, 1927.

⁴ W. A. Kerr and H. H. Remmers, *The American Home Scale*, Chicago: Science Research Associates, 1942.

Index⁵ were the objective status inventories considered as criteria in the three comparisons.

A second paper dealt with the relations of the St scale to a number of other variables.⁶ Students obtaining high scores on St tended to be reserved and prudent in response to personal inquiry, more conventional, more secure, more intelligent, more extraverted, more proficient in mathematics and English, and to have fewer somatic complaints, more satisfactory over-all adjustment, higher scholastic aptitude, and superior academic achievement; these trends were not marked, however, for the correlations ranged from .20 to .39 (although all were statistically significant). The pattern of correlations between St and the other variables was highly similar to the pattern between an objective status inventory and the same variables.

Neither of the two previous papers devoted any attention to the question of discrepancies between scores on St and on the various conventional indices of status which were employed. These latter instruments all specified manifest status-linked factors in their content, such as owning a car, having a fireplace in the home, etc.; for this reason they may be referred to as measures of "objective" status. The St scale may then be referred to as an index of "subjective" or "personality" status, for it does not specify any concrete material objects in its content which would indicate what the scale as a whole is measuring. Scores on objective status inventories, as thus defined, would largely be limited to the present level of material goods and possessions, and would be relatively uninfluenced by factors of potential social mobility and change, assuming that the scale was completed honestly. The St scale, on the other hand, would reflect present status level to the extent that it is a valid predictor of scores on an objective

status inventory, but because of the kind of questions it incorporates would be especially susceptible to deviations from such a level in the case of potentially upwardly- or downwardly-mobile persons.

A high school student, for example, coming from a poor home, but upwardly mobile, would receive a social status rating commensurate with his present home level on an objective scale. On the St scale, however, he might respond in such a way as to claim the social skills and other characteristics embodied in higher status answering. This would result in a different score, one equally as revealing as the objective status score, but one which would be classified only as an error of measurement if considerations such as these were not reviewed. In the light of such a possibility it would appear profitable to investigate the deviations between objective and subjective status scores as not merely chance errors, but as possessing meaning and significance in their own right.

The factor which would be most readily identifiable as an attenuator of the correlation between the two kinds of scales, then, is social mobility. Persons of low objective status, but upwardly mobile, might be expected to attain St scores relatively higher than on objective status. Conversely, persons of high objective status, but downwardly mobile, might be expected to secure lower St scores than would otherwise be predicted.

In order to examine the possibilities along this line, the various comparisons and correlations reported below were carried out. The first step was to convert both the objective and subjective status scores into comparable units, and then to obtain a discrepancy score for each subject by subtracting St from the objective status. The discrepancy scores were also translated into standard scores for ease in categorizing. The difference scores were considered both with and without regard to sign, but the results were significant only in the former case; hence, findings are reported only for the distribution taking account of sign.

The first evidence pertaining to discrepancy scores comes from personal descriptions supplied by the principal of the high school

⁵ Harrison G. Gough, "A Short Social Status Inventory," *Journal of Educational Psychology*, XL (Jan. 1949), 52-56.

⁶ Harrison G. Gough, "A New Dimension of Status. II. Relationship of the St Scale to Other Variables," *American Sociological Review*, XIII (Oct. 1948), 534-537.

in which the students were enrolled. A number of students were selected, illustrating several combinations of St and the American Home Scale (AHS), that is, both high, both low, AHS higher than St, and St higher than AHS. The principal was requested to write a brief statement about each student named, but was not informed until later of the basis for selection, or the objectives of the study. Typical examples of these personality portraits are given below.

Case 1. Male, high school senior. St 69, AHS 70. "The father is the manager of a local business firm. It is a fine family as far as I know. The boy has done a great deal of church work. The boy's one fault is his frequent tardiness. He is always courteous, but does have a mind of his own. He is very ambitious. At the present time he is trying for a scholarship to X University, which may be a little too high a mark for him."

Case 4. Male, high school senior. St 35, AHS 40. "This boy comes from the farm. He is shy and has an inferiority complex. He finds it hard to meet people. He is on the part-time program and his employer speaks very highly of him except for this one thing. He feels that he is overcoming this complex as he gains experience and more confidence in himself."

Case 6. Female, high school senior. St 31, AHS 36. "She is also from the farm. She is very quiet, works hard at her work, and follows regulations faithfully. It is not easy to learn to know her. Probably has somewhat of an inferiority complex also."

Case 9. Male, high school senior. St 40, AHS 77. "The father is a professional man. All of the boys in the family appear to be poorly disciplined. This boy is definitely a law unto himself. He is disrespectful, he is unmannerly, and he flouts authority. He does not excel in any line, and although he scoffs at the idea, I think he would like to. He is not generally accepted by the student body. He moves in a clique of five or six boys who are more or less discipline problems."

Case 12. Female, high school senior. St 64, AHS, 40. "Her mother is dead and the father has recently remarried. A year or two ago she was bold, boisterous, and aggressive. She was the wrong type of leader and somewhat of a problem case in her sophomore year. Since that time, however, she has improved in every respect. She has come a long way, due possibly to the influence of the stepmother."

These five cases are fairly representative of the sixteen summaries given by the principal. Where both objective and subjective status are high the student is seen as coming from a good family, as being ambitious, dependable, and potentially successful. Where both are low, the student is seen as lacking confidence and drive, and as being obedient and compliant. There was a tendency for students with AHS markedly higher than St to be described as lacking in responsibility and dependability, and for those with St greater than AHS to be characterized as more upwardly mobile; students with high St and low AHS scores tended to give signs of friction with their environmental backgrounds, and were not as acquiescent and submissive as those with St and AHS both below average.

The next step was to compare groups consonant in objective status, but differing in tangible evidence of mobility aspirations. Each student was asked how large a yearly income he anticipated at age 45 (or for girls, how large an income a husband would be expected to be earning). Answers were categorized into five steps: up to \$2,000; \$2-3,000; \$3-4,000; \$4-5,000; and \$5,000 and over. From the students who ranked below the class average on the American Home Scale, two groups were selected: those expecting up to \$3,000 annual income, and those expecting over \$4,000. The former group was considered to be stationary in status aspirations, and the latter group was considered to have given indications of potential upward mobility. These two groups were compared on several variables, as indicated in Table I.

The two groups are very similar insofar as objective status is concerned, but the group anticipating greater future income attained a somewhat higher mean on the St scale, in support of the hypothesis previously outlined. On the Ac scale,⁷ which is comprised of personality test items predictive of academic achievement but unrelated to intelligence, the two groups did not differ signifi-

⁷ Harrison G. Gough, "Factors Influencing the Academic Achievement of High School Students," *Journal of Educational Psychology*, 1949 (in press).

TABLE 1. COMPARISON OF STUDENTS WITH AMERICAN HOME SCALE SCORES IN THE LOWER HALF OF THEIR CLASS WHO EXPECT TO BE EARNING UP TO \$3,000 ANNUALLY AT AGE 45, VERSUS THOSE WHO EXPECT TO BE EARNING \$4,000 OR MORE, ON THE FOLLOWING VARIABLES: ST, ACHIEVEMENT (AC), AHS, CALIFORNIA ANTI-SEMITISM SCALE (A-S), OTIS IQ., HIGH SCHOOL HONOR POINT RATIO (HPR), THE MINNESOTA MULTIPHASIC PERSONALITY INVENTORY DEPRESSION SCALE (D), AND THE DISCREPANCY SCORE AHS-ST

	Up to \$3,000			Over \$4,000			Diff.	t		
	N-54		M	N-26		SD				
	M	SD		M	SD					
St	45.91	8.62	49.96	8.68	—	4.05	1.93			
Ac	49.15	9.82	46.31	8.87	—	2.84	1.23			
AHS	42.13	4.99	42.35	4.81	—	0.22	0.18			
A-S	196.41	52.98	183.88	62.35	—	12.53	0.92			
IQ	100.19	9.75	103.77	10.26	—	3.58	1.49			
HPR	1.94	0.61	2.01	0.77	—	0.06	0.37			
D	54.26	10.46	49.62	12.67	—	4.64	1.71			
AHS-St	46.20	9.56	42.38	8.58	—	3.82	1.71			

cantly. The differences were also insignificant on the Levinson-Sanford anti-Semitism scale,⁸ intelligence, honor point ratio, the depression scale of the Minnesota Multiphasic Personality Inventory,⁹ and on the discrepancy index, AHS-St. It might be added, however, that the lower score on AHS-St for the group expecting larger incomes approaches the five per cent level (probability approximately .09), and is in agreement with the hypothesis being studied.

Students from higher objective status were next considered to determine whether any of the factors would relate to potential down-

ward mobility. With these students expectations of \$4,000 or over were considered to be signs of stationary mobility, and of lesser annual incomes at age 45 as indications of possible downward mobility. Table 2 lists the same variables as were considered in Table 1.

It appears from this table that, although all students used in the comparisons were drawn from the upper portion of the objective status distribution, the two groups are nevertheless discriminated on this basis. Those students anticipating lower incomes are lower on both objective and subjective status. The difficulty in equating the groups on objective status makes any evaluation of the discrepancy score ambiguous, but the St scale taken alone continues to predict

⁸ Daniel J. Levinson and R. Nevitt Sanford, "A Scale for the Measurement of anti-Semitism," *Journal of Psychology*, XVII (April 1944), 339-370.

⁹ S. R. Hathaway and J. C. McKinley, *op. cit.*

TABLE 2. STUDENTS WITH AMERICAN HOME SCALE T-SCORES OF 58 AND OVER WHO EXPECT UP TO \$4,000 ANNUAL INCOME AT AGE 45 COMPARED WITH THOSE WHO EXPECT OVER \$4,000

	Up to \$4,000			Over \$4,000			Diff.	t		
	N-22		M	N-29		SD				
	M	SD		M	SD					
St	53.64	6.69	61.07	8.67	—	7.43	3.27			
Ac	53.68	8.02	49.55	11.54	—	4.13	1.41			
AHS	62.36	4.41	68.93	8.72	—	6.57	3.17			
A-S	160.23	49.16	139.69	56.99	—	20.54	1.32			
IQ	110.95	12.07	115.34	11.38	—	4.39	1.30			
HPR	2.62	0.69	2.59	0.62	—	0.03	0.16			
D	48.09	9.96	49.38	8.42	—	1.29	0.49			
AHS-St	59.05	8.50	58.31	10.85	—	0.74	0.26			

the mobility criterion.

It may be concluded from these two tables that students from poorer homes who anticipate relatively greater adult incomes than their classmates tend to secure higher St scores as well, and that among students from advantaged homes those who anticipate lower adult incomes tend to obtain lower St scores. Insofar as anticipated income can be taken as a sign of status constancy or status change, scores on the St scale do seem to be predictive of potential upward or downward mobility.

A better method of evaluating the meaningfulness of the deviations of St from objective status, perhaps, would be to select groups on the basis of discrepancy scores and then tabulate the actual numbers within each group who show the signs of higher status aspirations enumerated. The results of several analyses of this kind are presented in Tables 3, 4, and 5.

It can be seen at once from Table 3 that there is a tendency for students manifesting higher status aspirations (annual income of \$4,000 or more) to appear more frequently in the column of predicted upward mobility according to the discrepancy scores.

The prediction that from students of higher objective status those having lower personality status scores will give more signs of potential downward mobility is corroborated by the findings reported in Table 4.

These findings with respect to anticipated income levels were also tested in a new high school class, using a different measure of objective status, the Home Index. Utilizing the same relationships between objective and subjective status to select upwardly mobile and stationary students from the lower half of the objective status distribution, the expected income levels were tallied for the two groups. This comparison, offered in Table 5, again yields a chi-square probability approaching the five per cent level, and reveals a tendency for those predicted to be upwardly mobile according to the hypothesis being considered actually to give tangible signs of such aspirations on their part.

TABLE 3. THE NUMBER OF STUDENTS WITH AHS-ST T-SCORES OF 40 OR BELOW (PREDICTED UPWARD MOBILITY), AND OF THOSE WITH AHS-ST T-SCORES OF 55 OR MORE (STATIONARY) WHO ANTICIPATE VARIOUS LEVELS OF ANNUAL INCOME AT AGE 45; BOTH SAMPLES DRAWN FROM THE GROUP OF STUDENTS HAVING AHS T-SCORES IN THE LOWER HALF OF THE CLASS

	Upwardly mobile	Stationary
Expect up to \$3,000	12	13
\$3,000 to \$4,000	14	13
\$4,000 and over	12	2
	$\chi^2 = 5.84$	
	d.f. = 2	
	P = 0.055	

TABLE 4. THE NUMBER OF STUDENTS WITH ST T-SCORES OF LESS THAN 60 (PREDICTED DOWNWARD MOBILITY), AND OF THOSE WITH ST T-SCORES OF 60 OR MORE (STATIONARY) WHO ANTICIPATE VARIOUS LEVELS OF ANNUAL INCOME AT AGE 45; BOTH SAMPLES DRAWN FROM THE GROUP OF STUDENTS HAVING AHS T-SCORES OF 60 OR MORE

	Downwardly mobile	Stationary
Expect up to \$4,000	8	4
Expect over \$4,000	6	17
	$\chi^2 = 5.42$	
	d.f. = 1	
	P = 0.02	

TABLE 5. THE NUMBER OF STUDENTS WITH HOME INDEX MINUS ST DISCREPANCY SCORES INDICATING UPWARD MOBILITY, AND OF THOSE WITH HI-ST DISCREPANCY SCORES SUGGESTIVE OF A STATIONARY POSITION WHO ANTICIPATE VARIOUS LEVELS OF INCOME AT AGE 45; BOTH SAMPLES DRAWN FROM SUBJECTS SCORING IN THE LOWER HALF OF THE HOME INDEX DISTRIBUTION

	Upwardly mobile	Stationary
Expect up to \$3,000	17	32
Expect over \$3,000	22	22
	$\chi^2 = 3.09$	
	d.f. = 1	
	P = 0.08	

An incidental observation was also made concerning a notion advanced by Ruesch.¹⁰ Ruesch's hypothesis was that persons who tend to disagree with their parents also tend to reverse the mobility direction of their parents. In this study, students of initially low status who disagree with their parents would be expected to be more upwardly mobile than those who did not disagree, according to the Ruesch hypothesis. The students were asked to check the degree of agreement-disagreement with their parents

scores) was greater than .5; the chi-square probability arising from the comparison of the agree-disagree dichotomy with expected level of income at age 45 was also greater than .5. These results are unfavorable to Ruesch's hypothesis, but of course do not constitute a crucial test of the notion.

A final step in the analysis was to consider the correlations of the AHS-St difference scores with a number of other variables. It should be mentioned that there is some tendency for the discrepancy score distribu-

TABLE 6. INTERCORRELATIONS BETWEEN THE DISCREPANCY SCORE, AHS-ST, AND THE FOLLOWING VARIABLES: MINNESOTA MULTIPHASIC PERSONALITY INVENTORY (15 SCALES); SECURITY-INSECURITY INVENTORY; AMERICAN HOME SCALE; OTIS I.Q.; AND THREE-YEAR HIGH SCHOOL HONOR POINT RATIO.
(BASED ON 271 HIGH SCHOOL SENIORS; 144 BOYS, 127 GIRLS)

	r	SE		r	SE
MMPI scales: L	.00	.06	Security-Insecurity	.17	.06
K	-.20	.06			
F	.04	.06			
Hs	.01	.06	American Home Scale	.57	.06
D	.10	.06			
Hy	-.07	.06	Otis I.Q.	.08	.06
Pd	-.07	.06			
(boys) Mf	.06	.08	Honor point ratio	.08	.06
(girls) Mf	-.02	.09			
Pa	.10	.06			
Pt	.16	.06			
Sc	.02	.06			
Ma	-.04	.06			
Si	.16	.06			
St	-.44	.06			
Ac	.24	.06			

on a five point scale; a cutting point on this scale divided the group into those who agreed always or usually versus those who agreed half of the time, less than half of the time, or almost never.

Two comparisons were then made within the subgroup of students with objective status scores in the lower half of the distribution. The chi-square probability between the agree-disagree dichotomy and the upward-stationary mobility categories (dichotomized according to AHS-St discrepancy

tion to be attenuated by factors of regression; that is, a subject getting a high AHS score will tend, on the average, to obtain a somewhat lower St score. This causes the discrepancy score used in the present study to afford a less efficient index of prediction than might be obtained from an optimum combination of AHS and St. Table 6 gives the intercorrelations and standard errors.

Considering those correlations significant at or beyond the one per cent level, it appears that stationary, or downwardly mobile tendencies (higher discrepancy scores) are related to a more frank and unguarded response to personal inquiry; to a tendency toward worrying and ready admission of personal problems and complaints such as is

¹⁰ Jurgen Ruesch, "Social Technique, Social Status, and Social Change in Illness," in Clyde Kluckhohn and Henry A. Murray (Editors), *Personality in Nature, Society, and Culture*, New York: Knopf, 1948, pp. 117-130 (p. 125).

indicated by higher scores on the MMPI Pt scale; to greater social introversion; to higher scores on the Ac scale, which contains items predictive of academic achievement but unrelated to intelligence; and to greater insecurity.¹¹ (The correlations with St and AHS are largely direct consequences of the method of obtaining the difference scores.) None of these correlations is high enough, however, to permit any of the above statements to retain anything more than a very slight and highly general verifiability; certainly it would be extremely hazardous to apply such descriptions to individuals on the basis of this evidence.

¹¹ Maslow's scale was used, see: Albert H. Maslow, *et al.*, "A Clinically Derived Test for Measuring Psychological Security-Insecurity," *Journal of General Psychology*, XXXIII (1945), 21-41; also, Harrison G. Gough, "A Note on the Security-Insecurity Test," *Journal of Social Psychology*, XXVIII (1948), 257-261.

SUMMARY

A set of personality items was previously derived each of which reliably predicted scores on objective status inventories, and which, when considered as a scale, correlated approximately .50 with such inventories in test samples. Because of the nature of these items, in distinction to the content of conventional status inventories, it was felt that all deviations between personality status and objective status measurements were not merely errors, but were, on the contrary, to some extent meaningful deviations predictive of potential changes in status position.

A number of comparisons and inquiries did tend to substantiate this hypothesis. The relationship of objective-subjective status discrepancy scores, obtained by subtracting personality status from objective status T-scores, to a number of other variables was also investigated.

THE MYTHS OF HOUSING REFORM*

JOHN P. DEAN

Cornell University

THE STORY of housing reform is shot through with controversy, emotion, and pseudo-science. The smoke generated out of this controversy has concealed the economic and political manipulations of various groups seeking to maneuver the situation. The private housing industry has its folklore in terms of "Own-Your-Own-Homes," and "Filtering Down"; the chief medicine men of housing reform have now developed their own folklore; both groups make all the necessary passes and incantations before the public. In the meantime, the patient continues to sicken.

This situation arises from the recurrent dilemma of social reform movements: a set of conditions widely condemned is brought to public attention—in this case unhealthy and unsafe housing. But to remove these

conditions would be to change established ways of doing things. L. K. Frank pointed out as far back as 1925: "The crux of the problem is to find some way of avoiding the undesirable consequences of established laws, institutions, and social practices, without changing those established laws, etc."¹

Social reformers have long realized that restrictive municipal codes clinging to the margin where health and safety are threatened can change operating practices barely enough to achieve minimum protection for the family. To achieve broader social objectives, reformers embody these objectives in housing standards that would require considerable change in existing institutions.

Once codified, such housing standards give a point of leverage from which to pry at

¹ Frank, L. K., "Social Problems," *American Journal of Sociology*, January, 1925, p. 467.

* Manuscript received January 14, 1949.

existing housing regulations. Much of the fight for better housing revolves around the battle to narrow the gap between currently enforced regulations and reformers' standards of social welfare. Any achievement results mainly in a reformulation of the welfare standards to give a new margin for "progress." In a double sense the reformers are the "standard bearers" in the battle of the slums.

Frequently, however, reformers are hard pressed to justify these higher standards, since they represent debatable value-judgments and difficult scientific analyses. Therefore, the fight for social welfare through housing reform appeals to science and business realities where it can, but readily falls back on emotional appeals and myth where its defenses are weak.

By and large, these non-rational defenses of housing reform are of four kinds: (a) appeals to the subjective evaluational roots of housing standards, (b) myths surrounding the complex relationship of slums to social disorder, (c) myths about the social effects of rehousing and (d) myths about the financial liability of slums to the municipality.

(a) *Appeals to the subjective evaluational roots of housing standards.* Beyond considerations of health and safety, a housing standard embodying social objectives is likely to represent some personal yardstick or subjective judgment among the professional and technical people who fight for better housing. A subjective criterion is of little help in justifying legislation in the political arena. So, to bolster their position, housing reformers naturally appeal to the sympathies of good-hearted people by devices such as dramatic photographs of the squalor of slum dwellings. These techniques frequently pull a few heart and purse strings. Photographs of this sort, statistics on the lack of plumbing, spot maps of social disorders, and other similar trappings have become institutionalized as standard paraphernalia of housing reports.

(b) *The myths surrounding complex relationship of slums to social disorder.* But since it is often considered "emotional" to dramatize the unpleasantness of the slums,

attention is devoted to "proving" that slums impair other social values by causing crime and delinquency, ill health, or exorbitant municipal expenditures. These studies grew rapidly in the 1930's and helped to rally support for local housing reforms; they were quoted by high federal officials in the halls of Congress in support of legislation to implement slum clearance; they achieved a currency that entitled them to be quoted by the best authorities.

Most housing reformers probably believe that the effect of substandard housing on social welfare has been determined. But even with workable criteria for discriminating standard from substandard housing, an enormous amount of research time and effort would be needed to untangle the complicated causal relations. And one could probably never say "substandard housing contributes this much to delinquency, that much to poor health."

A number of commonly-used indices of social conditions and social pathology correlate with slum or blighted areas. By one or another study, the areas of most substandard housing have been found linked with:

- high population density
- high death rates
- high proportion of families dependent on social assistance
- high proportion of illiteracy
- high proportion of women employed
- high juvenile delinquency rates
- high rate of sex offenses
- high rate of gambling arrests
- more multi-family dwellings
- large average family size
- high proportion of males to females
- small proportion of owner-occupied homes
- high proportion of relief cases
- more unemployment
- more poverty
- high rates of divorce
- high rates of non-support cases
- high rates of illegitimacy
- high rates of venereal disease
- high rates of alcoholism
- low proportion of males married

- high proportion of foreign born
- high suicide rate
- high rates for various mental disorders
- low marriage rates
- high residential mobility
- more restaurants per 1,000 population
- low average educational level
- low proportion of radio ownership
- high rates of mental deficiency
- low proportion of telephones

In city after city—Buffalo, Birmingham, Cleveland, Denver, Detroit, Hartford, Indianapolis, Los Angeles, Milwaukee, Newark, Washington, and others—slum areas have been shown to be the areas of poorest health and the greatest personal and social disorder.² The implication is: "Remove the slums and you remove the social ills!" But it would be just as illogical to say that ills of slum areas are caused not by substandard housing conditions, but by the absence of telephone service, which also correlates with indexes of social disorder. Scientifically, we should not attribute causation to one or a few factors in an *area* which happens to be correlated with an overall *rate* of disorder. This blend of correlation does not explain why many families living in substandard conditions do *not* experience divorce, or delinquency, or alcoholism. It fails, further, to explain why in a given family in substandard housing one boy may be delinquent, or mentally deficient, or unhealthy, while his brother has remained free of these maladies. Beyond these few expected relationships between slum dwellings and health, the effect of poor housing becomes quite difficult to determine, especially where *social* behavior is involved. Goldfeld's study³ tested one

relationship with greater care than most studies. In the East Harlem slum area he held social factors constant where possible and compared the delinquency rates among families that lived in the superior structures with the rates among families that lived in inferior structures. He stated after careful statistical manipulation: "The one unmistakable conclusion that emerges from the study is that there is no relationship between bad housing in its physical aspects and juvenile delinquency as revealed by court records."

(c) *Myths surrounding Social Effects of Rehousing.* In their eagerness to get slum clearance legislation, housing reformers fought against the idea that slum conditions were the fault of the families that lived in the slums: How could the tenants be responsible for housing deficiencies such as lack of air, light, toilet, bath, or central heating? The reformers argued further that ill-health, uncleanliness, and delinquency are not *inmate* characteristics, but are the result of life in the slum environment. Their conception of slum environment emphasized primarily the inadequate *physical* environment of the slums. It understressed the connection between the *social* environment of the slums and the disorders they wanted to cure. So it was easy to jump to the conclusion that slum clearance would remove the social ills—and the public housing program has been sold with that assumption.

Social reformers hoped that the USHA would introduce a new way of life for the slum family. They believed that many personal and social maladjustments would wither away in the new life of the housing community. But they failed to appreciate the problems of rehousing. Public housing developments, once completed, have provided a laboratory for testing out the housing gospel that "decent housing instead of slums means less crime, less juvenile delinquency, lower costs for police and fire protection; it also means better health, lower

² A good summary of these studies appears in Part III of the *Cost of Slums*, a pamphlet prepared for the Housing Authority of the City of Newark by Dr. Jay Rumney and Sara Shuman (Newark, 1946, pp. 32 ff.). Also see: Mowrer, E. R., *Disorganization: Personal and Social*, Phila., 1942; Faris, R. E. L. and Dunham, H. W. *Mental Disorders in Urban Areas*, Chicago, 1939; Shaw, C. L. and McKay, *Juvenile Delinquency and Urban Areas*, Chicago 1942; Ford, James, *Slums-Housing*, Cambridge, 1936, 2 vol.

³ Goldfeld, Abraham, "Substandard Housing as

a Potential Factor in Juvenile Delinquency in a Local Area in New York City," abstract of Ph.D. Thesis, N.Y. University, 1937 (reprint).

death rates, and lower costs of medical care."⁴

Studies in Gary, Newark, Philadelphia, and Pittsburgh⁵ have shown that rehoused slum families have relatively low rates of tuberculosis, infant mortality, adult criminal offenses, juvenile delinquency, fires, home accidents, and communicable childhood diseases. But we cannot tell whether or not the families admitted to the projects were different in personal and social characteristics that would account for the lower rates. Low-rent public housing projects (where these studies took place) admit only families from substandard dwellings who are citizens of the United States, have low incomes, and no substantial assets. Single persons and oversized families are not accommodated. Tenant selection procedures draw proportionately more families with children or about to have them, more of the reliable or steadily employed and fewer trouble makers. Public assistance cases are accepted but kept to a limited proportion. Families that apply for admission are also "self-selected" according to their knowledge of available public housing and their willingness to move in, if approved. Some families are undoubtedly deterred by such considerations as: home ownership, satisfaction with present quarters, unwillingness to leave their present neighborhood, their preference for a store-dwelling combination, or misconceptions or prejudices against "Government housing."

The extent of self-selection in determining the "effective" public housing market is indicated in the results of a pilot survey in

⁴ Myer, Dillon S., Commissioner of the Federal Public Housing Authority, at the *Hearings before the Committee on Banking and Currency, U. S. Senate, 80th Congress, on S. 866*, Gov't. Printing Office, Washington, 1947, p. 118.

⁵ Housing Authority of the City of Gary (Indiana), *A Report*, Gary, 1945; Housing Authority of the City of Newark (N.J.) *The Social Effects of Public Housing*, Newark, 1944; Phila. Housing Authority, *Homes for War Workers and Families of Low Income*, Phila., 1943, and Bureau of Social Research, Federation of Social Agencies of Pittsburgh and Allegheny Counties. *Juvenile Delinquency in Public Housing*, 1944, and *Vital Statistics of Public Housing Residents*, 1945.

deteriorated areas of Aliquippa, Beaver Falls, and several other small steel towns in Beaver County, Pennsylvania, in May 1946. The 1,194 interviews revealed that the families eligible and willing to move in amounted to only six per cent of the families interviewed, only 14 per cent of the families in substandard housing conditions. Only 31 per cent of all *eligible* families said they were willing to move in. This willingness of families to move into public housing varies sharply with condition of dwelling, tenure, and income, as the table shows. Compared with an equivalent number of dwellings in slum or blighted areas, public housing developments undoubtedly include fewer aged indigents, transients, single men and women, hobos, crackpots, criminals, panhandlers, prostitutes, alcoholics, bohemians, taxi-dancers, and other social misfits. Little wonder that indexes of social-welfare favor public housing.

A few studies have made thoughtful attempts to discover whether families transplanted from the slums to public housing developments improve in health and social life:

(1) Full medical data were secured for seventeen tuberculous families admitted to public housing in New Haven and resident there for three to seven years. They were matched with seventeen parallel families on the date of primary diagnosis, the status of the disease at the time of diagnosis, and, so far as possible, on age, sex, and racial characteristics. While only nine of the control group showed favorable progress, fifteen of the rehoused families progressed.⁶

(2) For 317 families resident in a public housing development in New Haven for $2\frac{1}{2}$ to $4\frac{1}{2}$ years, the rate of juvenile delinquency per 100 children per year was only 1.64 as compared with a rate of 3.18 for those same families during the seventeen years before entrance to the project.⁷

⁶ Barer, Naomi, "A Note on Tuberculosis Among Residents of a Housing Project," *Public Housing*, August 1945, p. 133.

⁷ Barer, Naomi, "Delinquency Before, After Admission to New Haven Housing Development," *Journal of Housing*, Dec. 1945-Jan. 1946, p. 27.

Answers to the Question: "If you had the chance to move into a public housing project, and if you had to pay no more rent than you are now paying, would you move in or not?"

Owner-occupied			Tenant-occupied					
	Standard	Substandard	Standard		Substandard			
Weekly Income \$40 and Over	Weekly Income Under \$40	#1 #2 and #3	Rent \$25 and Over	Rent Under \$25	#1	#2	#3	
Cases— 206	72	191	68	119	52	198	102	29
100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
Yes— 8.3%	13.9%	11.0%	17.6%	26.1%	42.3%	40.9%	53.9%	82.8%
No— 81.0%	73.6%	79.1%	57.4%	59.6%	48.1%	40.4%	30.4%	13.8%
Don't Know— 10.7%	12.5%	9.9%	25.0%	14.3%	9.6%	18.7%	15.7%	3.4%

Source: Special tabulation of the interview schedules of an income survey of small towns in Beaver County, Pa., conducted by the Housing Authority of the County of Beaver, in cooperation with the Federal Public Housing Authority.

Depending on whether none, any one, any two, or all three of the following conditions were present:

- in need of major repairs
- without private bath for exclusive use
- more than 1.5 persons per room

The dwelling unit was considered:

- standard = none
- substandard #1 = any one
- substandard #2 = any two
- substandard #3 = all three

(3) Forty-four families about to be admitted to public housing were matched on ten characteristics with 38 families who were on the waiting list to be admitted and both groups were given tests on sociometric scales on morale, general adjustment, social participation, and social status before the experimental families moved in in 1939 and a year later in 1940 after the experimental families moved in. The results showed no significant change in morale for either group. Both groups gained in social participation, but the rehoused group gained twice as much. Both groups gained in social status, but the rehoused group showed a greater gain. The scores on condition of living room showed a striking gain for the rehoused group, but a loss for the control group. Both

groups improved in percentage "use crowded," but the rehoused group improved three times as much.⁸

d. *Myths surrounding the dollars-and-cents costs of slum dwellings to the municipality.* In all, about a dozen studies have been made to show that slums are a financial liability to the city. The methods for showing the deficit vary widely, but in every case (1) municipal expenditures must be allocated to substandard areas, and (2) assumptions must be made about the relation of bad housing to those expenditures. The cost of public baths may be counted against

⁸ Chapin, F. Stuart, "An Experiment on the Social Effects of Good Housing," *American Sociological Review*, December 1940, pp. 868-79.

dwellings where bath facilities are lacking, but it may be impossible to tell how much was spent on patrons from a given area. Other costs, such as arrests for burglary or prostitution, are usually assigned against the slums where the offenders live, thus ignoring arbitrarily that this protection may benefit the people in better residential areas. Most studies assign to slum areas costs that might be attributed to the population composition of those areas. The well known "Cost of the Slums in Newark" study, for example, assumes that the cost of educating a child is the same for a slum as for a good residential area. It then assigns to the slum area a per capita cost for education that is double the per capita cost for education in the good residential area on the grounds that in the slum area "20 per cent of the population consisted of children attending public schools whereas only 10 per cent of the population of (the good residential area) were in that category." Before assessing the costs of these additional school children against the slums, the Newark Housing Authority might have reflected on the figures in its earlier study showing that (1) the proportion of school age children and (2) the birth-rates per 1,000 women 15-44 years of age are both substantially higher in its public housing projects than in substandard areas it compared these projects to.

If studies of the costs of slums were to throw out all expenditures that either are not allocable by area or are related to population composition rather than to substandard housing, much of the so-called financial deficit of the slums would disappear. And surely, any municipal contribution to the cost of rehousing (which in 1944 ran over \$5.00 per unit per month in public housing nationwide) would overbalance any municipal expenditures chargeable directly to the substandardness of the dwellings.

With these myths, the humanitarian fight for housing reform has opposed the traditional mores that maintain "business-as-usual" attitudes toward the slums. Public housing, as a new reform institution, represents a sharp and unconventional departure in operation of residential real estate: "Gov-

ernment" on both local and federal levels becomes directly concerned with the construction, financing, ownership, and operation of housing. But even though public housing has spread from community to community, it is clear that the battle for social welfare through housing is far from won. Without question, public housing has provided rehoused families with structures more comfortable and decent (Standard I) and more healthful and safe (Standard II) to live in; but the anticipated improvements in social welfare (Standard III) have failed to materialize. Studies that show striking gains in social welfare have generally fallen into some unwarranted manipulation of the facts; they are inconclusive at best.

This failure should not surprise us: public housing operations have been hedged in and hampered in the pursuit of social welfare objectives by several traditions: (a) the heritage of laissez-faire individualism, (b) the current practices of real estate, of local government, and of federal government, (c) housing reformers' confusion about the social effects of rehousing.

a. *The Heritage of Laissez-faire Individualism.* Public housing reflects the conditioning atmosphere of those aspects of protestant ethic and the spirit of capitalism that underwrite (1) the sanctity of private enterprise, (2) the debilitating effect of "charity," and (3) the moral qualifications for public aid. Each of these is reflected in the operation of public housing:

(1) The American faith in the sanctity of private enterprise is reflected in the increasing clarity with which public housing formulates its sphere of activity as non-competitive with private enterprise. Public housing permits occupancy only by families of low-income who are unable to afford decent available private quarters. "Low-income" has frequently been interpreted at levels well below the minimum budgetary requirements for working class families (as defined in welfare assistance budgets). In New York City, the admission limits for veterans to a post-war project were originally set so low that many of the young families admitted found that without the assistance

of the in-laws they had been doubled-up with, they were quite unable to make ends meet. A family that exceeds the income limits in public housing is expected to vacate even though the increase in income is unlikely to continue. The breaking of local friendships and community ties makes stable community life an objective hard to achieve.

(2) Public housing has operated under the stress of the American folk belief that "any individual worth his salt can get ahead and raise his family out of the slums." By this reasoning a slum family is morally inadequate and must not be pauperized by public assistance which makes things too easy. This belief in "minimum charity" is translated into accommodations that are often pared to the bone in room size, equipment, noise insulation, cupboards, and floor covering. A government study reported that many units required about \$85 to bring the dwelling up to tenants' standards.

(3) Public housing reflects the widespread American attitude that help should be given only to the "deserving." Thus benefits are limited to citizens and to families of more than one person. Since slum children, forced to live in the slums through no fault of their own, deserve a better break, tenant selection procedures generally cater to families with children. Few accommodations are provided for the aged and the proportion of public assistance cases is limited. And since those who help themselves are more "deserving" of help, the steadier, more reliable, less troublesome families are generally favored.

b. *The current practices of real estate, local and federal government.* (1) Public housing operation reflects, too, private real estate customs. The rent in private dwellings is determined by the size and "quality" of the accommodations; public housing emphasizes that families should pay according to their ability. Many rent schedules in current operation in public housing are a curious amalgam of these two points of view; in some schedules the rent is determined by both *income* (ability to pay) and *number of bedrooms* (size of accommodations). Furthermore, the private landlord

concerns himself only with property maintenance and rent collection; public housing following the example of private practice has concentrated on these as its primary duties and left social considerations in the background.

(2) Public housing operation reflects its status as an adjunct of the local government. Local housing authorities were set up as organizations separate from the city administration proper to avoid political interference. And for the most part, graft or favoritism have been negligible in land acquisition, contract awards, tenant selection, and job-filling. Occasionally, the services for a project (such as utility contracts or orders for management supplies) have been manipulated in favor of one local son rather than another. Sometimes the top positions have been filled by political favorites, but sometimes, too, this guaranteed needed political support. But when local political connections have been emphasized at the expense of contacts with local social service organizations, the long-run social welfare objectives of public housing have been hampered. In dealing with these problems the supervising Federal agency has walked a tightrope between local autonomy and a too-close supervision of—or "interference" in—local affairs.

(3) Public housing must gear itself to the practices of federal politics (and in New York, state politics). Since legislators are often under attack for over-spending and since their housing attitudes have usually been derived from private real estate practices, political experience has encouraged local housing authorities to emphasize the efficiency of its real estate operation rather than the extension of its achievements in social welfare.

c. *Housing reformers' confusion about the social effects of rehousing.* The housing reformers themselves have frequently been taken in by their own myth: "Clear the slums and you remove the social ills." Those who are well trained in community organization and leadership have never thought of clearing the slums and providing in their place decent, safe and sanitary homes as the whole public housing job. But the vast bulk

of housing officials are not well trained in the arts of organization and leadership. Faced with the concrete realities of rent collection, property maintenance, and budget balancing, they have not been in a position to make the most of the opportunity rehousing offers to inaugurate social relationships, to help families to participate in community life, to encourage the use of social services infrequently or never used, and to create leisure time satisfactions which exceed anything previously experienced. Until housing officials frankly face the rehousing of families as a *social experiment* in relocation and adjust policies and procedures accordingly, large-scale rehousing operations will frequently be accompanied by conditions falling far short of current objectives of social welfare.

Fortunately, a few scattered housing research studies point in this direction: both R. K. Merton's Studies of Craftown and

Hilltown and M.I.T.'s Group Dynamics' Study of a Veteran's Housing Development for Students⁸ approached housing developments as unusual opportunities to study social organization. The New School's Research Center for Human Relations has underway a study of intergroup relations in several housing developments with varying patterns of Negro-White occupancy. If a few studies such as these can demonstrate the decisive impact of haphazard architectural decisions and of operating policies and practices arrived at by loose judgment, then perhaps we can look forward to a widening circle of housing reformers who argue their case *not* in terms of myth but in terms of housing's real possibilities for social reform and sociological experiment.

⁸Both studies are still in manuscript, as yet unavailable.

THE INTERACTION OF ETHNIC GROUPS: A CASE STUDY OF INDIANS AND WHITES*

BURT W. AGINSKY†

The City College of New York

THE WORLD population is confronted with the intensification of racial, cultural, religious, and national conflicts. That is, there has been an intensification

of intercultural relations of populational groups.

Of late years worldwide travel, trade,

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† This article is a summary presentation of a chapter of a book now in process. A similar paper, "Intercultural Relations: A Case Study of Indians and Whites," was read by the author at the Annual Meeting of the Eastern Sociological Society, New York City, 1947.

Research was begun by the author on this area in 1932. Field research was begun in 1934 and was continued in 1935 and again in 1936. This was done in conjunction with the Social Science Research Council of Columbia University under the supervision of the Anthropology Department. The first joint social science research was begun in 1939 under supervision of the Social Science Field Laboratory (B. W. Aginsky and E. G. Aginsky, Directors) in conjunction with New York University Graduate School, and was continued in 1940 and

again in 1941, when it was interrupted by the War. The field work was resumed in 1946. This was made possible by a grant from the Viking Fund. The Social Science Field Laboratory was reactivated in 1947 primarily as a result of an additional grant by the Viking Fund. The 1947 field research was done in conjunction with the Maxwell Graduate School of Syracuse University. It was continued during the summer of 1948. A total of thirty-two scientists representing the various social sciences have co-operated in this integrated study of a community. (Cf. bibliography at the end of this article for further publications on this study by the author.)

Thanks are due especially to the Viking Fund, the Social Science Research Council, the Social Science Research Council of Columbia University, and the many private donors who have made the work possible.

communication, migration, and especially war and the threat of war have been important in bringing this situation about. Whether we war or not, we must deal with populations, as, for example, in our governing of Japan and Germany at the present time and our negotiations with them previous to the war. The same is true in relation to the USSR.

Whether we discuss the problem of inter-group relations upon the international level (for example, the United States and Russia), the national level (for example, whites and Negroes in America), the industrial level of management and labor, or the level of a small community, as we shall in this paper, the point at issue is that of participation.

Are there restrictions in relation to the members of either group? Is there partial, limited, or negative participation? Is the participation social, economic, religious, political, or educational, or is the participation in relation to area and/or mobility? Is the participation tabooed, tolerated, or welcomed?

This paper touches upon the positive values of diverse cultures in a great complex where each sub-population contributes to the total functioning culture.

In brief, the point is: to what extent shall members of sub-populational groups participate in the world, or in a nation, or in an industry, or in a small community, whatever the case may be?

Members of the Social Science Field Laboratory have been engaged in a field research study of this subject since 1934. Thirty-two men and women representing the various social sciences have participated in the project. They have endeavored to reconstruct the history of the community, but more importantly have concentrated upon the process of change as it occurred.

THE PRE-WHITE SITUATION

The original inhabitants of the area, the center of which lies approximately 110 miles north of San Francisco, were the Pomo Indians who lived a sedentary life in a small territory in northern California. Previous to white contact these people were proud and

self-sufficient. They had a well-established form of government and social control which pervaded their entire social organization. They had definite forms of family life, religion, war, manufacture, commerce, occupations, play groups, and an economic system based upon a bi-metallic currency, as well as the other components generally found in the culture of a self-perpetuating population of long standing.

Professions, occupations, and various forms of leadership were honored and required a lifetime of training to insure success. Participation in all aspects of the culture was primarily upon the basis of ability. Every individual had access to the honored walks of life. The only differentiating factor was that of sex.

THE PERIOD OF FIRST WHITE CONTACT

During the first period of contact with the Spaniards and Americans these Indians were slaughtered, enslaved, and indiscriminately confined upon reservations (today we call them concentration camps) without regard to family, tribe, or other affiliations. The land was expropriated. This was a period of disruption and disintegration, with no opportunity for self-government or self-sufficiency.

The Indians were kept at Missions and later on reservations. They were not allowed to participate in their traditional occupations and other forms which had meaning in their lives. Mobility was non-existent. Participation was under the strict supervision of those who were in charge.

The Russians who had preceded the Spaniards married Indian women as did some of the white Americans.

THE SITUATION IN 1934

In 1934, the date of our first field trip to the area, we found a community composed of a majority of whites, most of whom applied stereotypes to the minority group: "The Indians are dirty, lazy, the lowest Indians in the USA. They are Diggers. They get drunk, kill one another," and so on and on.

The Indians lived on rancherias, each

some miles from a city. Their settlements looked forlorn, their houses were unpainted. In general they resembled "the other side of the tracks."

The Indians were allowed only in the balcony of the movie house, were served only in one restaurant (that owned by a Chinese), and had but a short time previously won a case in court to have their children attend the same schools as the white children. They were agricultural workers who were paid upon the basis of piece work. They had poor work habits. All persons of both sexes participated with little expectation of ever achieving any higher status. They received no appreciable rewards. There was no greater participation to which to look forward. The majority of the Indians were distinct in dress and action when in town, wearing a type of early frontier dress. Infants were carried in baby-baskets on their mothers' backs.

THE PRESENT PERIOD

During the past three summers we observed many changes. It is almost impossible to differentiate the Indians from the whites in town, except that more whites than Indians have fair skins. Many of their houses are freshly painted; some have lawns. Many have washing machines, oil furnaces, and radios. The women have permanents, wear slacks, and wheel their babies in baby carriages, while the men wear sport coats and slacks. A large proportion of the old Indians have died off. The majority of the young Indians have gone to school, many have graduated from high school, and some are attending college. A few have positions in town, something unheard of during our first visit. Every Indian in the community has increased his mobility tremendously. It is the exceptional Indian who has not been to San Francisco, Sacramento, and even more distant places. A large number of them were in the Armed Forces. Many of the women worked in the shipyards and other war industries along with men who did not get into uniform.

Some Indian men dress, act, and speak like Mexicans in order to participate more

freely in the purchase and drinking of alcoholic beverages.

TYPES OF SOLUTIONS

This is the historical picture in brief.

Three types of solutions to interpopulational participation have been tested specifically in this community during the study: 1) amalgamation, 2) cultural pluralism, and 3) dominance-submission.

1) *Amalgamation.* This solution includes intermarriage and complete intermingling along with full participation. We found that both the whites and the Indians were opposed to this as an immediate solution. They referred back to the Russian, Spanish, and American cases where intermarriage had occurred. They stated that the Russians took the women away and that it just did not work with the Spaniards or Americans. They deeply resented the prospect of any relatives marrying out. All descendants of intermarriage could participate only as Indians unless they could hide their Indian ancestry.

2) *Cultural Pluralism.* Both Indians and whites accepted this solution verbally, with few exceptions. The whites frequently stated, "Let the Indians stay on their rancherias and we'll stay where we are." In actual practice the ranchers want a reservoir of highly skilled labor available for agricultural pursuits. In general, the storekeepers want the Indian trade, as do the gasoline-station owners and other business people. A few religious groups desire partial participation, but this is primarily upon a segregative basis. The summer of 1947 found the Mormons engaged in an active campaign to bring the Indians into their church. It will be interesting to study the results of this in the future. There are members of the community who desire participation by the Indians upon an equal basis. A few whites treat the Indians as pawns in community affairs.

The majority of the Indians do not care to have whites visit their homes. But the Indians would like to participate to a greater extent in the community despite the fact that they also say they desire cultural

pluralism. On their part they have consistently obtained a wider variety of participation and a greater amount of it. They still desire more. By court action and threats of court action they now sit anywhere in the theater, eat in the various restaurants, and go to the beauty parlors in town. On the other hand, many Indians resent white participation when the Indians have their dances and "big times." Others like it, but very little is done by the Indians to get the whites to participate. This has changed slightly of late with remarkably good results primarily due to the efforts of the Indian women through their "Pomo Mothers Club."

3) *Dominance-Submission.* This solution is accepted verbally by a majority of the older white residents, many of whom state, "Why don't those Diggers know their place? They shouldn't mix with the white man. All they're good for is to work in the fields." The Indians resent this attitude deeply. A number of them in ordinary conversation with us, and the majority of them when drunk, complained bitterly. They said, "Those whites treat us worse than dogs. They let their dog in the house, but not us. They took our land away; now all they want us to do is to work for them. When there is a decent job they'll give it to any old white bum before they'll give it to one of us." It should be emphasized at this time that many Indians place the blame for their position upon themselves because there have been many cases where Indians have been undependable or have gotten drunk while in positions of authority and responsibility.

We have found only a few cases where the Indian considers the white superior.

Our study has pointed up the fact that the dominance-submission solution of the early days has resulted in much disintegration of personality, especially among the younger Indians who have lived in outside communities, have attended schools away from the community, or have been in the Armed Services. The condition in the community has resulted in heightened aggression among the individuals; and since the aggression cannot be resolved against the

whites who are felt to be the cause of it, the Indians fall into a pattern of escapism marked by drunken brawls, killings, juvenile delinquency, brittle marriages, and also sporadic trips to the outside world.

These three major types of solutions can be compared upon the basis of participation. Amalgamation means total participation. Cultural pluralism means negative participation. Dominance-submission means exploitative or restrictive participation. (In some other cases the quota system according to the wishes of the dominant group is the type of participation found.)

In the early days of contact the Indians were allowed to participate in agriculture, the herding of cattle and sheep, and other work because the whites needed them as labor. Later, when the Indians had "hard money" they were allowed to participate in the business establishments as tolerated customers. Now they are participating in the restaurants, the amusement and recreational facilities, education, transportation, the beauty parlors, and many other facilities. For the most part they are merely tolerated participants, but there definitely has been an increase in the quality and quantity of participation.

REASONS FOR THE CHANGES

The change came about upon the basis of at least three factors. (A) Culture. In the first place the Indians raised themselves by their bootstraps. They imitated the whites and became less distinguishable in language, dress, and behavior. (B) Population. In the second place the older members of both the whites and Indians died off while the white population increased due to immigration. This diluted the diehards on both sides, especially amongst the whites where today the majority know little about the Indians. (C) Mobility. Thirdly, increased mobility of the Indians and participation in larger geographical areas enabled them to get away from the oppression and to deal intimately with whites in other areas. This brought about a decrease in the tension among the Indians. They knew they could get away. It also brought about much assimilation. The

increased mobility, both physical and social, was of the greatest importance in the change.

It is also important to note that although the laws (re: participation in schools, movies, etc.) were on the statute books, they meant little to the Indians until they took matters to court and had the laws invoked. This meant that the Indians had to learn about the laws, how to get their cases through the courts, and how to get the backing of some of the whites in the community before starting the proceedings. All of the above makes clear the fact that the Indians had been participating to a greater and greater extent through the years.

PARALLEL CASES

The change in participation by the Pomo Indians is paralleled by the case of the Negroes in America moving to the North and then visiting or returning to the South. The same is true of the Mexicans and other groups. Each case contains multiple factors and qualitative as well as quantitative differences. However, in general, the similarities are sufficiently clear.

The concomitant of restrictive mobility found in the world today, coupled with internal pressure due to expanding populations, is a major problem. Whether the population has been compressed from a large area into a small area as in the case of the Pomo Indians, or whether a population has increased from 100,000,000 to 400,000,000 as in the case of India, aggression, frustration, and other psychological manifestations are present. The DP camps of Germany are another case in point. It is recognized that the individuals are in an extreme situation and that mobility is the solution. If the mobility factor is kept at zero or close to zero, and participation is confined, we can expect what has been reported, namely, intensified pressure with the accompanying disintegration of the personalities of the individuals.

Cultural pluralism, whether it be on the intimate community level of the area we have presented, the national level in the Negro-white situation in the South or that of the Jews and Arabs in Palestine, the industrial level in the management-labor

situation, or the international level in the case of Russia and the United States, cannot be accepted as the only solution. Neither will dominance-submission be successful. Amalgamation has been rejected as the immediate solution.

FUNCTIONALLY INTEGRATED PARTICIPATION

In the case of this community we have found that the Indians during the aboriginal days had developed a handicraft of the most beautiful basketry known in the world. This skill has been continued up to the present time. The Indians also had done a certain amount of clay figurines and some small amount of wood carving, as well as leather work.

At the present time their work is seasonal (agricultural), with the majority of the year unproductive.

The community is on the famous Redwood Highway and derives a tremendous amount of profit from the tourist trade. The tourists stop overnight, which is the only participation found. If it were possible for a program to be initiated whereby the Indians would use the products of their white neighbors, i.e., wool, skins, etc., to manufacture art objects for the tourist trade, it is very possible that the rewards which they would receive would be incentive to bring about a high level of interest. In turn it might follow that the whites would honor the Indians for their handicrafts. This plan has been used in other cases to mutual advantage.

Another open area in which neither the whites nor the Indians are involved at the present time and where the Indians could be of service to the community is in the field of bricks and hollow tile, both of which are badly needed by the community. There are other open areas which might be entered by the Indians. Thus in each case the Indians would become more integrated in a functional fashion with the total community, with the possibility of mutual benefits.

It is essential that each case be carefully studied by the community itself in order to achieve success in progressive integration and greater participation. In this community there is already a certain amount of func-

tionally integrated participation. If the Indians were to be deleted from the community, it would be confronted with a shortage of highly skilled agricultural workers, and the stores and other institutions would be deprived of a large revenue. The dislocation would be tremendous.

Thus the consciousness of the total community in relation to functional integration is of prime importance in successfully bringing about greater participation.

On all levels the only apparently workable solution is that of a functionally integrated participation. This means that the fields, aspects, and areas of participation must be charted and known. The amount of participation, whether it be total, negative, or partial, must be understood. Change must occur upon the basis of the acceptance and the rejection of participation by both groups. Negotiation is fundamental especially in relation to the change in participation, both qualitatively and quantitatively, as time passes.

It is possible to chart what could be called the open areas so that participation by the minority group does not meet with opposition. It is also possible to ascertain how participation in new aspects would bring about cooperation from the dominant group while aiding the submerged group to attain a position where they would gain respect and thus the possibility of greater participation in the community.

Competition leading to participation in the same kinds of occupations and industries should continue, but from the many cases studied thus far we know that it is a long tedious process which by itself has not, as yet, been wholly successful.

Whether there be a conscious plan with conscious change of the formal structure by laws or a gradual change by increased participation that is informal and without plan, it must be recognized as such.

Increased participation is essential for mutual respect and for self-respect. In the case of the Pomo Indians who had their participation in the total community cut down to almost zero and who then became almost what the whites said they were, this was due to the lack of participation. When

participation increased, both the Indians and whites remarked about the change. In fact, many of the whites spoke with pride about how "the Indians had improved."

Cultural pluralism is present and will continue to some extent. However, the negative aspects of cultural pluralism can be eliminated and thus result in a functionally integrated participation where the contributions of each group are recognized and appreciated by both the majority and the minority groupings in the community. In this way the negative attitudes of both populations involved can be decreased to the point where full participation of the individual, with markedly decreased tension and friction, based upon a mutual desire for that full participation, can be achieved. But it must be done consciously.

In cases where both groups are opposed to the three types of solutions, there remains but one realistic program, namely, a conscious program of ever-increasing functionally integrated participation.

This is the fundamental basis of democracy. The opposite brings aggression, frustration, waste, and inefficiency, whether it be on the community level, the international level, or any level in between.

ADDITIONAL PAPERS BY THE AUTHOR OF THIS STUDY

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RACE PATTERNS AND PREJUDICE IN PUERTO RICO*

MAXINE W. GORDON

c/o Argonne National Laboratories, Chicago, Illinois

LYNCHINGS, race riots, and race beatings do not occur in Puerto Rico; cases of rape ascribed to race are unknown. This has led to a widespread belief that race prejudice is absent on the island. Contrasts between Puerto Rico and other regions of marked race friction might lend credence to this belief were it not for the island's growing concern over race problems. Although this concern has sharpened in recent times, evidence is easily obtained to demonstrate a more than contemporary interest in the topic.

Early as well as recent Puerto Rican writers have claimed that race prejudice in Puerto Rico does not exist—except in "social" or "class" areas. José Celso Barbosa makes this point in his *Problema de Razas*.¹ More recently, Tomás Blanco² and Pablo Morales Otero³ support the same view. In 1945, the Insular Board of Vocational Education published a pamphlet in which the same claim is made.⁴ The view that race prejudice does not exist in Puerto Rico is shared by some American writers:⁵ "Racial discrimination becomes virtually impossible, since a majority of the people are of mixed Indian, white, and Negro blood".⁶ Our purpose in this article is to examine some of the historical and cultural factors contributing

to and underlying the belief that there is no race prejudice in Puerto Rico. We will also present the major characteristics of Puerto Rico's race problem.

Negro slaves were first introduced into Puerto Rico in 1510. Since then only six Negro uprisings were recorded over a period of more than 350 years—all of minor character.⁷ Slavery was abolished in 1873, and today Puerto Rico celebrates as an island holiday the *Día de las Razas*: the freeing of the slaves.⁸ The absence of violence in race relations in Puerto Rico is an enviable record. Not all aspects of Puerto Rican race relations are equally enviable, however. It is with these that our analysis is primarily concerned.

Puerto Rico's leading newspaper, *El Mundo*, recently published the findings of President Truman's Civil Rights Committee on race prejudice in Hawaii, Puerto Rico, and other U.S. dependencies.⁹ This committee reported race discrimination in the employment of Negroes on the island. This was vigorously denied by numerous Puerto Ricans, and the report was cited as additional evidence of continental ignorance of the "true conditions" in Puerto Rico. It is admitted that the average American knows little of Puerto Rican affairs. However, in the preceding year, the same newspaper had published a protest by Puerto Rican students against discriminatory practices in the employment of Negro students in certain offices of the University of Puerto Rico.¹⁰ It was later claimed that this complaint was used as a political bludgeon against the University administration. Whatever the purpose

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¹ *La obra de José Celso Barbosa: Problema de Razas, Documentos para la Historia, Seleccionados y Recopilados por Pilar Barbosa de Rosario*, 1937, Vol. 3, p. 31.

² *El Prejuicio Racial*, 1942, p. 63.

³ *Nuestros Problemas*, 1945, p. 195.

⁴ *Socio-Economic Conditions in Puerto Rico Affecting Family Life*, p. 69.

⁵ The term "American" is used throughout this report to refer to citizens of the continental United States, a designation common in Puerto Rico. It should be borne in mind, however, that Puerto Ricans, as citizens of the United States, are also "Americans."

⁶ E. S. Garver and E. B. Fincher, *Puerto Rico: Unsolved Problem*, 1945, p. 21.

⁷ Vincenzo Petrullo, *Puerto Rican Paradox*, 1947, p. 19.

⁸ March 22; it is interesting to note that the United States does not accord its own Negro emancipation the same degree of public recognition.

⁹ October 30, 1947, p. 1.

¹⁰ October 29, 1946, p. 3.

behind the protest, it can be asserted that many students as well as informed persons outside the University felt the complaint to be a valid one. This is not a reflection on any prejudicial employment policy of the University of Puerto Rico. But it may be taken as evidence of the absence of any expressed policy *against* such practices should they exist within the University. Moreover, since student and staff members of the University are drawn from essentially the same social and economic levels of Puerto Rican society, it can safely be assumed that the attitudes of one reflect those of the other.

We can personally testify that areas exist on the campuses of both the University and its branch at Mayagüez which are referred to by students as "Broadway" and "Harlem."¹¹ Negro students are found in the "Harlem" areas; white or non-Negro students frequent the campus "Broadways." The sorority-fraternity system of both University and College further characterizes this separation. These organizations in relation to race discrimination have been discussed in detail by Renzo Sereno in his article "Cryptomelanism, A Study of Color Relations and Personal Insecurity in Puerto Rico."¹² It is perhaps significant to note here that the University does not offer any courses dealing specifically with race prejudice, race and cultural relations, or minority groups. Courses in the Social Science Department which include these topics are regarded by many faculty members and administrators as necessary evils not above suspicion. Currently, one administrator has stated that "Too much attention is being paid to race on the campus."

This would be less surprising were it not for the frequent denial that problems of race exist. E. B. Reuter, commenting on the "... common assertion . . . that there is no racial prejudice" in Puerto Rico says: "There is a failure to recognize or an un-

willingness to discuss underlying causes."¹³ We not only agree with this statement, but point out that there is "an unwillingness to discuss" *any aspect of race* with Americans. The Puerto Rican is justified in his suspicion that the average American does not share his views on race, and that to many he is an "inferior." We have in mind the well-known statement by James Bryce: "In Latin America, whoever is not black is white; in teutonic America, whoever is not white is black."¹⁴ This statement is far from accurate, but it does represent the extremes in two opposing views on race.

Clarence Senior, in his study *Puerto Rican Emigration*,¹⁵ indirectly calls attention to the race problem in Puerto Rico when he shows the Puerto Rican's reluctance to emigrate to the United States for fear of race discrimination. This implies the Puerto Rican's belief in the absence or rarity of race prejudice on the island. However, Senior remarks later in the same report that there is a ". . . widespread adoption of the color prejudices of continentals" (by Puerto Ricans).¹⁶ Another author comments as follows: "Color prejudice, then, relatively unknown on the islands, prevents the Puerto Ricans from migrating to the States. . ."¹⁷ As a Puerto Rican who read these two statements once said to us, "Why don't they make up their minds?"

Many colored Puerto Ricans, fearing the same treatment accorded colored continentals, are anxious to separate themselves from the latter, both in Puerto Rico and on the mainland. This has been pointed out by Tomás Blanco,¹⁸ and earlier by Lawrence Chenault in his study, *The Puerto Rican Migrant in New York City*.¹⁹ The practice of separation is particularly evident in the "Spanish Harlem" of New York City, to

¹¹ "Culture Contacts in Puerto Rico," *American Journal of Sociology*, September, 1946, p. 98.

¹² *The American Commonwealth*, 1910, Vol. 2, p. 555.

¹³ 1947, p. 31.

¹⁴ *Ibid.*, p. 33; italics ours.

¹⁵ Vincenzo Petrullo, *op. cit.*, p. 23; italics ours.

¹⁶ *op. cit.*, p. 42.

¹⁷ 1938, p. 150.

¹¹ The College of Agriculture and Mechanic Arts is located at the western end of the island in the city of Mayagüez, approximately 90 miles from the University.

¹² *Psychiatry*, August, 1946, p. 261-269.

which large numbers of Puerto Ricans have migrated.²⁰ Here the colored Puerto Rican displays his different food habits, his Spanish name, and his knowledge of "Latin" music, the Spanish language, and the "tropics" as evidence of a *Latin* rather than *Negro* background. This sets up a many-sided conflict, one which enables us to demonstrate the considerably intricate, and often contrasted, race-values of Puerto Rico and the United States. First, it engenders internal conflict between colored New Yorker and colored Puerto Rican in New York's Harlem—as they compete with each other for "white" social and economic privileges. Second, because the Puerto Rican Negro in New York exemplifies "Latin" characteristics, it serves to identify all Puerto Ricans as Negroes to both Negro and white native New Yorkers.²¹ This is far-reaching in its effect, since it breeds animosity between the Negro Puerto Rican immigrant in New York and the white Puerto Rican in *Puerto Rico*. The latter, always conscious of his reputation on the continent, is incensed at being regarded as Negro. He insists that "only the worst type of Puerto Rican emigrates to New York." Third, it emphasizes the race conflict in Puerto Rico between white and Negro Puerto Ricans. Anxious to redeem himself in continental eyes, the white Puerto Rican withdraws discreetly—but further—from the Negro islander. When he visits the mainland, the white Puerto Rican avoids the Puerto Rican "Harlems" in New York or elsewhere. In the relative immunity of an unknown outsider, he may even divorce himself completely from his fellow white Puerto Ricans and identify himself as "Spanish" or "Latin," both terms ambiguous to the average continental. Fourth, since the white Puerto Rican is

never sure of his status with continentals, he denies relationship to the Negro wherever he finds him: in Puerto Rico, on the continent, in the Caribbean, or elsewhere in Latin America. More significantly, he denies any tinge of Negro blood he himself may knowingly but secretly possess. The white Puerto Rican has a weighty burden: he must be "white" not only to white continentals, but also to other "white" Puerto Ricans who know only too well that he may fear some almost but never entirely forgotten *raja*. (In Spanish, *raja* means "stripe"; it is used in Puerto Rico to indicate a tinge of Negro blood.)

In the face of these maneuvers for status by his fellow white Puerto Rican, the Negro Puerto Rican is—or has been until recently—largely silent. It is this fact, more than any other, which puzzles and not infrequently irritates or even exasperates the "liberal" outsider or the Puerto Rican who recognizes the contradictions involved in these attitudes. We hope to explain this later as a product of Puerto Rico's own cultural development, a factor not always considered by the "reformer" or the proponent of race equality who comes to Puerto Rico from outside.

The material so far presented should reveal a configuration of race relations not apparent to the casual eye. Those who believe race prejudice does not exist in Puerto Rico have seen Puerto Rico, we feel, with a casual and uncritical eye. As evidence that the race problem of the island has a larger significance, and a relationship to race problems in other Latin American areas, we quote the following:

"It is said that in Brazil there is no race problem for the colored man. An effort is made to scatter far and wide before the winds of propaganda the idea that here in Brazil the Negro has found his paradise, where he may enjoy equal rights with other men. Don't believe this . . . if the race drama here does not take the form of bellicosity and physical clashes, that does not mean that it does not exist. It is something that exists psychologically for a great part of the population—this veiled racial discrimination, mystified among the prop-

²⁰ "Puerto Rican Migration," Bureau of Applied Social Research, Columbia University, 1948; this report reveals that between 160,000 and 200,000 Puerto Ricans reside in New York City.

²¹ The attitude of the New Yorker may be taken as representative of attitudes in other U. S. areas to which large numbers of Puerto Ricans have migrated.

positions of a constitution which defines all men equal before the law."²²

The migration of continental Negroes to Puerto Rico is negligible, but U.S.-born Negroes of Puerto Rican extraction frequently retain family ties with colored islanders. Increased transportation facilities between the U.S. and Puerto Rico have made movement between these two points relatively easy, as evidenced by the recent heavy migration of Puerto Ricans to New York City (more than one-third of the total number of Puerto Rican migrants to this area have arrived since the end of World War II). Unless family ties have been intimate, however, the continental-born Negro of Puerto Rican parentage will find himself unwanted should he return to the island. If he is poor, he competes for jobs on the already overloaded Puerto Rican unskilled labor market. If he is rich or has professional standing, he is still a competitor, either for professional jobs, or for the social status attributed to such positions. Culturally foreign, he encounters the same reception given the U.S. Negro who visits or lives in the island. This fosters a special resentment on the part of the "Americanized" Negro of Puerto Rican extraction, both in Puerto Rico and in the United States. Finding no desirable place for himself in the multi-valued color scheme, he is hostile to white American, white Puerto Rican, Negro Puerto Rican, and continental Negro—with none of whom he is fully identified.

The white Puerto Rican recognizes—privately, if not in public—the struggle of the colored islander against race prejudice. He not infrequently regards it as a threat to his own standing as "white." In his frustration, he is often hostile to the same white American whose recognition he desires. He is never quite sure with which of the two to lodge the greater portion of blame for his feeling of insecurity: the Puerto Rican Negro or the white American. Such diver-

gent behavior-patterns are readily manifested in Puerto Rico; one observer has even identified them by the term "cryptomelanic"—the fear of, and effort to hide, the color problem within one's self.²³ Against this background, the position of the *recognized* Puerto Rican mulatto is even more anomalous. He is subject to inter-personal problems of considerable psychological significance, particularly when race prejudice operates within the intimate family group. We hope to make this clear when we later develop the topic of inter-racial marriage. For the present we are mainly interested in showing that race and race prejudice are dominant concerns in Puerto Rico, particularly since they are both unacknowledged and denied.

As another index to the degree of interest in this topic on the island, we cite five radio-broadcasts recently heard in Puerto Rico. These broadcasts, delivered by both Puerto Rican and continental speakers (one an anthropologist), discussed race prejudice on both the island and the mainland in an attempt to correct prevalent misconceptions of race. Mimeographed copies of the broadcasts were later issued under the title "El Prejuicio de Razas."²⁴

Not all steps to clarify the race problem in Puerto Rico have been effective ones. Plans were lately made to study a "typical white" and a "typical Negro" community in the island. This study was designed to show that Negro and white Puerto Ricans have essentially the same way of life, a culture which has grown out of common problems and achievements of the Puerto Rican people as a whole. The claim of "superiority"—racial or cultural—of the white race on the island could thus be challenged. Although laudable in aim, this project completely ignores the mulatto element of Puerto Rico's population. Race mixture has characterized Puerto Rico from early times, and references to it are easily discoverable in early and current literature dealing with the island.²⁵

²² Statement of Abdias de Nascimento, prominent Brazilian theater-director; quoted in the Chicago edition of the *Pittsburgh Courier*, September 4, 1948, p. 7.

²³ Renzo Sereno, *op. cit.*

²⁴ Discusión de Mesa Redonda, February 22, 1947.

²⁵ Cf., Cayetano Coll y Toste, *Boletín Histórico*

Since no known "mulatto community" has ever developed in Puerto Rico, and since mulatto segregation does not exist, the possibility of finding the "typical white" and "typical Negro" communities necessary for the conditions of this study is doubtful. This does not, of course, rule out the admitted preponderance of Negro over white in certain sections of the island, notably the Eastern Coastal Region. The major slum areas of the island, however, are predominantly inhabited by large numbers of low-paid colored workers. An explanation of the economic factors contributing to this would possibly rob the project of its original purpose: to demonstrate that Negro and white Puerto Rican have the same way of life. In addition, we may question the validity of ascribing cultural growth in Puerto Rico or elsewhere to races—white or black—however humanitarian the motive. Previous attempts to explain world culture on this basis have been faced with the problem of crediting major cultural advances to the correct "race," particularly when such contributions were made in prehistoric times. We have no reason to believe that the foundations of Puerto Rican culture can be "racially" traced with any more facility, or that subsequent cultural development in the island can in this way be selectively categorized.

Studies such as that above, or others so oriented, face the additional problem in Puerto Rico of determining the racial identity of the Puerto Rican mulatto. Although both the topic of race mixture and the term mulatto itself are socially taboo in Puerto Rico, the mulatto is nevertheless a significant portion of the population—a portion which has not yet obtained biological, much less social, stability. As Eric Williams states, "One might be *pardo*, or *moreno*, or *trigueño*, indicating different shades of

de Puerto Rico, 1923, Vol. 12, p. 24; Tomás Blanco, *op. cit.*, p. 52; J. Colombán Rosario and Justina Carrón, *El Negro*, 1945; An extensive study of inter-racial marriage in Puerto Rico has appeared recently under the title "The Morality of Race Mixing in Puerto Rico," by Charles Rogler, *Social Forces*, October, 1946.

brown."²⁶ We are informed of numerous other terms which signify varieties of race mixture in the island: *grifo*, meaning persons of white skin with "kinky" hair of any color; *jabao*, meaning persons of white skin with *blond* "kinky" hair; and *prieto*, meaning "just plain black" showing no mixture. Finding in various degrees of rainfall an analogy to race mixture, the Puerto Rican "country people" may designate *aguacero* (hard rainfall) as persons very black, *lloviznas* (mild rainfall) as those medium black, and *opaco* (cloudy) as "trigueños bastante oscuro," i.e., persons with "kinky" hair but not completely dark skin. One may excusably find these designations interesting, particularly when it is realized that the "social eye" has made observational distinctions reasonably approximate to those of anthropological charts—and has further attributed to each a corresponding social status. It is therefore surprising to read such statements as: "Occasionally there is a mixture, but it is relatively rare, and there does not seem to be a special desire in that direction."²⁷ Nor does the latest official census of Puerto Rico (1940) recognize race mixture; it lists the population simply as 76.5% white and 23.5% Negro. That this does not accurately represent the Puerto Rican population is suggested by Sol Descartes in the recent island publication "Basic Statistics on Puerto Rico,"²⁸ and elsewhere by Charles Rogler,²⁹ Eric Williams,³⁰ and others.³¹ Moreover, according to the 1920 census of Puerto Rico, the mulatto element was *six times greater* than the Negro, and represented approximately 25% of the total island population. The absence of the mulatto in the 1940 census cannot be accounted for, we feel, by his real disappearance from the

²⁶ *The Negro in the Caribbean*, 1942, p. 62.

²⁷ Vincenzo Petrullo, *op. cit.*, p. 20.

²⁸ 1946, p. 1.

²⁹ *Comerio: A Study of a Puerto Rican Town*, 1940, p. 16.

³⁰ "Race Relations in Puerto Rico and the Virgin Islands," *Foreign Affairs*, January, 1945, p. 312.

³¹ Cf., previously cited works of Sereno, Chehault, and Rogler, 1946.

population—except as he “disappeared” into the census recordings.

We have evidence that the Puerto Rican is highly conscious of race mixture. This is provided by a well-known island song which asks this question: “And your grandmother, where is she?”³² The implication is that she is hidden—in the kitchen—because her skin is dark, although his (the mulatto to whom the song is addressed) “came out white.” Commenting on this, Sereno says, “As marriage and childbearing have occurred across every possible line, it can be stated that not only grandmothers, but also grandfathers and mothers and fathers are often hidden in Puerto Rican kitchens.”³³ Another song more recently recorded is *¿Blanco de Qué?*,³⁴ which says, in effect, “You may be white, but *what kind of white* (to what degree white) are you?” We mention also the melody *Angelitos Negros*,³⁵ in which the Puerto Rican Negro’s longing for social recognition is exemplified. Here the singer asks that paintings be made of “little black angels” as well as white ones, “Because all the good little Negroes also go to heaven.” This seems to us indisputable evidence of the Puerto Rican’s awareness of both race mixture and discrimination.³⁶ In addition we cite the recent report, *Some Situational Aspects of Race Relations in Puerto Rico*,³⁷ in which not only is race prejudice in Puerto Rico identified, but its quality and frequency of manifestation are correlated with various social situations.

The racial ambivalence and resultant anomalous status of the Puerto Rican mulatto can best be appreciated if three characteris-

³² Fortunato Vizcarrondo, “Y tu agüela, e ‘onne ejtá?”, first published as a poem in *Dinga y Mandinga: Poemas*, 1942, p. 32.

³³ *Op. cit.*, p. 265.

³⁴ Facundo Rivero; Verne Recording Company of America, Record No. v. 0321 B.

³⁵ By Manuel A. Maciste.

³⁶ The Puerto Rican newspaper, *El Mundo*, May 2, 1948, reports that a church in the island town of Salinas now exhibits paintings of both Negro and white angels, the work of a Puerto Rican painter, Júlio César Murillo de Santiago.

³⁷ Charles Rogler, *Social Forces*, October, 1948.

tics of Puerto Rican society are recognized. First, economic conditions of the island foster racial crossings. Second, some, *but not all*, of such crossings are socially condoned (through never encouraged). Third, the class mobility of the mulatto may operate independently of these two factors, determined by the “. . . tendency to define light mulattoes as white as they move by their achievement up the social scale.”³⁸ We will consider each of these points separately.

First: the economic conditions of Puerto Rico foster inter-racial unions because “crossing” takes place more frequently among the lower income groups, and because the vast majority of Puerto Ricans subsist on incomes which are notoriously low. One island publication which outlines these conditions refers in its title to the entire Puerto Rican population as “Three-fourths Ill-fed, Ill-clothed, Ill-housed.”³⁹ An accompanying condition is the “poor man’s” or consensual marriage. It is estimated that from 15 to 20% of the married population does not formalize its marriage by either State or Clergy. This provides us with another index to the degree of inter-racial marriage, since less stigma attaches to “crossing,” particularly if consensual and in the lower economic levels.⁴⁰

Second: although, as Rogler states, wealthy white Puerto Rican men marry mulatto women without apparent loss of status, their mulatto wives never achieve equal recognition. Social areas persist in which they do not feel at ease, or from which they well understand themselves to be “excused.” White women marrying mulatto or Negro men suffer a marked loss of status. Moreover, “. . . the available evidence indicates that three white men marry mulatto women

³⁸ Charles Rogler, *op. cit.*, 1946, p. 80.

³⁹ Published by the Office of Information for Puerto Rico, 1943.

⁴⁰ This does not excuse the mulatto offspring from the fear of illegitimacy and its associated social stigma. As Gunnar Myrdal says in *An American Dilemma*, 1944, p. 115, “. . . the fact should be taken into account that mixed offspring were almost always the result of illegitimate sex relations. . . .”

for every white woman who marries a mulatto man."⁴¹ This signifies that although men have more "social freedom" than women in sex-relationships (as in the United States), both are subject to a more than casual control in inter-racial affairs. In the United States, this "control" is legally operative in 29 states which retain laws prohibiting the marriage of whites and Negroes.

Third: although either Negro or mulatto Puerto Rican may "move up" the social scale by his personal achievement, his movement is always relative; his final social position does not equal that of the white of "similar achievement." In Puerto Rico, one prominent Negro politician has obtained considerable "recognition" by virtue of his political acumen and vote control over labor groups—no small proportion of which are "dark" Puerto Ricans. But it is common knowledge that although this man's political influence entitles him to considerable "recognition," his race prohibits him from full social acceptance in several élite circles. We know a Negro Puerto Rican sugar-consultant of considerable ability who has more prestige among sugar manufacturers *outside the island* than in Puerto Rico. Other examples known to us of discrimination against both Negroes and mulattoes of recognized ability are too numerous to include here.

It has been estimated that less than 40% of all the white Puerto Rican families can trace their ancestry with any assurance to a "white" background.⁴² Although the author admits this to be a "more than generous estimate," we question its validity still further since it "... comprehends not families biologically white, but the middle and upperclass families interested in their ancestry as a complement to their social status."⁴³ In our judgment, it would be extremely difficult to eliminate attitudes on race from such family records in Puerto Rico. Moreover, according to the special 1935 census of the island, 85% of the *colored* population was "mixed." We do not know what percentage of this group regards itself as

⁴¹ C. Rogler, *op. cit.*, 1946, p. 80.

⁴² R. Sereno, *op. cit.*, p. 263.

⁴³ *Ibid.*, p. 267.

white. Furthermore, the Negro population of the island is decreasing, and it is suggested elsewhere that if the present rate of decline continues, the Negro population will disappear in from 70 to 100 years.⁴⁴ This seems to us particularly significant since, in spite of the decline in Negro population, Puerto Rico has one of the highest known birth-rates in the world, and a population which doubles itself approximately every 30 years.

Contrary to the opinion of Eric Williams,⁴⁵ it is our belief that continued white discrimination against the Negro in Puerto Rico will aid, rather than prevent, the decline in Negro population. We conclude this for the following reasons: one, the difficulty of determining any biological color line between white and mulatto will permit the mulatto to "pass" as white—certainly on census records; two, the desire to "pass" will be increasingly nurtured by continued discrimination; and three, race-crossing is already a cultural tradition of some duration on the island. We believe that a consideration of race-crossing as a *cultural trait* would contribute substantially to an understanding of Puerto Rican race values, particularly since such considerations have been completely lacking in the past. Recently, two well-known scholars in the field of race relations have pointed to another Caribbean area where there is a "... need to take into account *traditional sanctions* no less than overt institutions."⁴⁶

As a further complication in Puerto Rican race mixture, Negro and white elements are not the only contributors to the Puerto Rican blood stream. One competent authority has noted, after more than 400 years since the island's discovery, that "... marked Indian features were casually observed everywhere, especially in the isolated mountainous regions."⁴⁷ Robert Morss Lovett, whose kindly attitude towards Puerto

⁴⁴ J. Colombán Rosario and Justina Carrión, *op. cit.*

⁴⁵ *Op. cit.*, 1945.

⁴⁶ Melville J. and Frances S. Herskovits, *Trinidad Village*, 1947, p. viii; italics ours.

⁴⁷ Jesse W. Fewkes, *Aborigines of Puerto Rico and Neighboring Islands*, 1907, p. 24.

Rico can hardly be questioned, speaks currently of "... the great mass of Puerto Rican peasantry, the *jíbaro*, a mixture of Spanish, Negro, and Indian strains, who constitute three-fourths of the inhabitants."⁴⁸ Even this has been turned to advantage by the race-conscious mulatto; he not infrequently speaks of himself as part *Indio*, rather than Negro. A current myth growing out of early historical records and former population distribution is also popular: that in the "remote interior" of the island, persons of "pure Spanish (i.e., *white*) blood" are predominant. This is hardly substantiated by empirical observation today. It should be noted further that in Puerto Rico the "remote interior" is only an approximate term since the total area of the island is less than that of the State of Connecticut. Moreover, population pressure (more than 600 persons per square mile) and considerable internal migration have made contact of races inevitable in all parts of the island. It is perhaps unfortunate that the "truly white" families in Puerto Rico are subjected to suspicion of their purity of origin. But by utilizing the same prejudicial techniques so deplored in others, they themselves become the servants of discrimination. The irony of this contra-

diction is apparent to but few.

Since Spain ceded Puerto Rico to the United States in 1898 at the close of the Spanish-American War, Puerto Rico has been in constant contact with American culture. The "American way of life" is now manifested in both material and non-material aspects of the Puerto Rican community. Many American attitudes which the Puerto Rican has acquired are being integrated only with extreme difficulty. For large portions of the Puerto Rican population, many continental attitudes towards race are not acceptable. Such differences produce conflict, and are symbolic of other incompatibilities between two cultures whose backgrounds and present components show wide variation.

Because the island has been virtually deluged with studies, plans, projects, and "Reports on Puerto Rico" to small benefit of its major problems, we conclude with the following emphasis: persons attempting to define and deal effectively with the race problems of the islands (American and Puerto Rican alike) must become increasingly aware of the importance of cultural differences. Otherwise, we see little reason to hope for either cultural stability in Puerto Rico or any lessening of the growing race conflict.

⁴⁸ *All Our Years*, 1948, p. 312.

OFFICIAL REPORTS *and* PROCEEDINGS



THE 1949 ANNUAL MEETING

It has been decided by the Administration Committee of the Society that the 1949 meeting will be held in New York City on December 28-30 in connection with the Allied Social Science Group which includes the American Economic Association, The American Political Science Association, the American Statistical Association and several others.

The Committee's decision thereby to ignore the advisory vote of the Executive Committee in favor of a meeting in Denver was strongly influenced by the fact that a report of the Reorganization Committee is expected to come before the Society at this meeting and it was felt that attendance at a Denver meeting would be so small that major policy decisions of the Society should not be taken under such cir-

cumstances. The Committee did, however, feel that a meeting west of the Mississippi should be arranged in the near future, probably in 1950.

The Program Committee has decided to organize the program about the topic "Frontiers of Research in Sociology." Areas of research will be chosen for particular consideration which are, so far as possible, representative of the major research interests of the members of the Society. The various section interests will be integrated with these. As soon as a detailed plan has been worked out, chairmen will be appointed to arrange the papers in each of the meetings planned. Members of the Society are invited to send suggestions to the Program Committee.

TALCOTT PARSONS
President

CURRENT ITEMS



NOTES ON RESEARCH AND TEACHING THE ASSAULT ON SOCIAL SCIENCE

GEORGE SIMPSON
The City College of New York

The behaviorist, neo-positivist school of sociology in America, with its emphasis upon mathematical symbolization, has gained many cohorts during the last quarter of a century, but it is only latterly that it has found a champion to present its tenets systematically and in full panoply. If in this paper we direct ourselves almost exclusively to one representative of this school which has so many followers, it is because his presentation of its case is both most recent and most thorough. Indeed, it should be a source of deep gratification that the position of this school of thought has finally been placed upon a full canvas, and been offered for popular showing. Only thus could both its merits and demerits be made obvious.

For two decades, in a series of publications long and short, Professor George Lundberg, erstwhile president of the American Sociological Society, has carried on a campaign for scientific method in the social sciences, and particularly sociology. The kind of world Lundberg wants is one in which the social scientist will be accepted as the final authority in informing men of how they can achieve the goals they set for themselves without himself approving or disapproving of these goals. "We have attempted to show that science, as the most effective instrument of achieving *whatever* goals we may pursue, should be welcomed by all men, however they may disagree on questions of final causation, the origin or the final destiny of man."¹ This campaign has now become so distinct to Lundberg that the delineaments can be stated in epigram and aphorism, simple enough to convey his meaning even unto the scientifically untutored. In his latest presentation, *Can Science Save Us?* he has made a case which the layman and the

beginner can understand and which is obviously the completion of a long struggle in the direction of a systematic position.

Properly to appraise where Lundberg and his followers stand, it is necessary to discuss certain major issues with which he deals. These issues have been raised in *Foundations of Sociology*; *Can Science Save Us?* and in an oblique sort of way in *Social Research*. The issues are as follows:

1. The nature of science.
2. Social research and social problems.
3. The training of social scientists.
4. Imputation of motives.
5. Values, social science, and the "good society."

We shall here follow the procedure of stating *seriatim* Lundberg's position on each issue with accompanying analysis and criticism.

1. *The nature of science.* To Lundberg science is "a technic of adjustment." It is the "accepted point of reference" for the achievement of "adjustment" which, he tells us, is "a word used to describe the situation under which the activities of an organism come to rest or equilibrium." The organism comes to rest or equilibrium when its tensions or imbalances are released.²

Here Lundberg's discourse appears to be a confusion of the logic of science with the psychological satisfaction which scientific explanation gives to the individual. If science achieves adjustment for the individual why does Lundberg feel it necessary to convert men to what will get them in equilibrium and what is to him "in our time and for some centuries to come, for better or for worse . . . to an increasing degree the accepted point of reference with respect to which the validity (Truth) of all knowledge is gauged."³ Men apparently are not yet ready to accept this "form of adjustment" as the way to ease their tensions and achieve satisfaction through getting the organism into adjustment with their environment. But that is because science as the corpus and method of solutions to human problems is upsetting to the

¹ *Can Science Save Us?*, p. 97.

² *Foundations of Sociology*, p. 5.

³ *Can Science Save Us?*, p. 36.

adjustments which men have made to their environment. For science is not a form of adjustment, but a critique of the human adjustments that have been made. The life of reason epitomized in its highest form by science is for Lundberg nothing more than an extension of biological adjustment. But if irrationality can adjust men to their "problems," why should they turn to science? Men resist scientific method because the human *organism* prefers to remain in equilibrium (adjustment for Lundberg) and not to overcome illusions. To give up the adjustments he has made, in return for science, man must become the critic of the adjustment process itself. The process is not self-critical; only reason rising above the inherent irrationality of the adjustment process can achieve that state where man is willing to accept the presence of folly as his own worst enemy. Murphy⁴ has stated: "To open one's eyes and see what is there to be seen, honestly, accurately and without the bias of preconception, prejudice or tradition, is an intellectual, and not merely a physiological achievement." To confuse the activity by which man triumphs over "adjustment" with adjustment itself is like identifying medicine with disease itself. Lundberg appears to conceive of the continuum of evolution as involving no qualitative discretion in the differentiation of species. Logic becomes a form of behaviorism and science the handmaiden of biology.

2. *Social research and social problems.* Lundberg cites as examples of good scientific work in sociology and the social sciences a piece of research by Stuart C. Dodd and one by Samuel A. Stouffer. "Under the auspices of Allied Force Headquarters, Stuart C. Dodd developed a polling organization for determining in the invaded areas facts regarding the behavior and conditions of life as well as opinion regarding such subjects as public security, crime and the mores governing its control, the people's satisfaction with governing officials, attitudes toward cobelligerency, status of shelter and clothing, food supply and distribution, etc." It was used in Sicily. "A survey indicated that very few people had received their sugar ration for five months. Thereupon the local officials were confronted with these facts and were told to get busy. A follow-up survey in a week showed the situation greatly improved and in two weeks practically corrected. Here we have a public which for the first time in years finds itself

consulted on such matters and then observes that its complaints actually bring results. Experience of this kind probably goes farther than any propaganda for democracy that could be invented."⁵

The other piece of research which Lundberg looks on with favor—that by Stouffer—appeared some years ago in the *American Sociological Review*. Stouffer studied the mobility habits of an urban population and arrived at the following generalization: "The number of persons going a given distance is directly proportional to the number of opportunities at that distance and inversely proportional to the number of intervening opportunities."⁶

Such individual pieces of research Lundberg feels will ultimately add up to a significant body of verified knowledge, and form a corpus of instruments for further research. "Men are patiently accumulating data about human behavior in a form which in the fullness of time will permit a type of generalization which has never before been possible. Important instruments have been invented in recent years for measuring opinion, status, social participation, and many phenomena of communication and interpersonal relations. Indeed, the invention and perfection of instruments for the more accurate and precise observation and recording of social phenomena must be regarded as among the most important developments in the social sciences."⁷

Lundberg's two instances of what he calls "basic scientific work" are apt to appear as trivia to the layman, who, Lundberg says, should learn to turn to the social scientist if he wants workable answers. A look at the present situation in Sicily and Italy which confronts this nation would show Lundberg that Dodd's investigation is a typical example of trying to take care of minutiae in the belief that policy will take care of itself. When the invasion of Europe began—as it did in Italy—some social scientists were convinced that a test of what kind of world we were going to get out of the war was in the offing. It was demanded that no traffic be had with Italian Fascists and that the common people of Italy be called upon to revolt, and that American money and men be thrown into a program of establishing democratic institutions there. It was pointed out that, failing this, civil war might be imminent and the Communist alternative might shape up as a

⁴ *Can Science Save Us?*, p. 38-9.

⁵ *Op. cit.*, p. 40-41.

⁶ *Op. cit.*, p. 22-3.

⁷ Murphy, Arthur E., *The Uses of Reason*, p. 34.

possibility. One hypothesis that might have been prosecuted was the effect that putting a Fascist general like Badoglio in power would have upon the Italian masses and on the growth of communism. Was the malfunctioning of the rationing system which Dodd investigated a part of the collapse of the Italian political system or did it just malfunction in a vacuum? Apparently social research accepts the creation of the general problem with social piety and then investigates the sugar rationing system on the assumption that we must accumulate many theses and thats in order to gain respect for the social scientist sufficient for him to be recognized as the arbiter of disputes and problems plaguing mankind. Until that happens, according to Lundberg, "The nations doubtless will continue to rage and the people to imagine vain things."⁸ Meanwhile, in the catacombs social scientists will painstakingly with slide-rule and frequency-table be investigating why nations rage and people imagine vain things; and when rage has subsided and vanity been overcome, the social scientist will emerge from his lair, chart in hand, to announce that if he had been listened to, rage would have been abolished and vanity relieved.

In the case of the law which Stouffer arrived at concerning the mobility-habits of an urban population, it is difficult to see how this differs from the simple declarative statement that people migrate until they find what they want. Stouffer studied numbers of cases of movement, showed that people did not go any further after they found what they were looking for, but did go further if they did not find what they were looking for, on the way. Mobility in urban areas offers a magnificent opportunity for studying the "rootless" character of American culture. Or to postulate the hypothesis of Robert S. Lynd:⁹ "Since urban living operates seriously at present to confuse and to devitalize our culture, science needs to discover ways to knit these loose population masses into living communities of interest, before this degenerating tendency renders the culture impotent." To apply Lynd's more general hypothesis to the problem which Stouffer was facing, it would be asked, Is mobility a search for opportunities or is it a sign of psychic restlessness, a searching for the evanescent new in a culture which permits little spontaneous expression on the part of its members?

⁸ *Op. cit.*, p. 115.

⁹ Lynd, R. S., *Knowledge for What?*, p. 244.

How does it happen that American culture in the form of the social sciences sets such great store by research like Dodd's and Stouffer's? All sociological inquiry would seem to be concerned with illuminating the structure and functioning of social relationships (human behavior oriented towards other individuals) in cultures. It is not necessarily concerned with helping a given type of structure or set of functions to keep going or strengthen its position relative to other structures or functions. That is, sociological research is not synonymous with "administrative research" or, *a fortiori*, with "market research." To do better what is already being done or to sell more goods or satisfy more consumers with the same goods does not add one iota to our understanding of the why and wherefore of modern administrative processes or modern conditions in the market. It does not tell us what kind of behavior our culture sets store by, nor hold this kind of behavior up to the ideals being professed as the intellectual and moral guides of behavior—the values to which we swear allegiance.

3. *The training of social scientists.* The basic integrating principle in education and particularly in the education of social scientists is, for Lundberg, "an understanding of the nature of scientific method as applied to human affairs."¹⁰ The humanities will be relegated to the status of aids to science and art. Contemporary science, he says, is simply the residue of the groping of all the ages.¹¹ Most of the "classics" will be relegated to the history of error; what we really need is "more and better classics regarding human behavior."¹² The training of social scientists will involve substituting for the language of folklore refined mathematical symbols. "Geometric, arithmetic, and algebraic ways of expressing relationships usually come with the maturity of every science. . . . The more intricate and variable is the situation we wish to describe the more dependent we become upon mathematical systems of symbolization. Sociology has for some time felt this need. . . . More recently much work has been done in the development of measuring instruments adapted to societal phenomena hitherto unmetrized. With the rapid developments in this field, the time is ripe for the systematization of the whole field of general sociology in quantitative symbols which, however unique the sub-

¹⁰ *Can Science Save Us?*, ch. iv.

¹¹ *Op. cit.*, ch. iv.

¹² *Op. cit.*, p. 75.

ject-matter to which they refer and however unmetrized as yet some of these phenomena may be, can nevertheless be manipulated according to the already established and tested rules of mathematics."¹³

Mathematical symbols and statistical techniques appear to be the criteria of the scientific to Lundberg. But to attack those problems most adaptable to certain techniques available is to cut the problem down to the size of the techniques, instead of lifting the techniques up to the size of the problem. There can be no sensible dispute concerning the great worth of advanced statistical methods properly applied. But proper application has to do with their pertinence to the problem under investigation. To decide upon pertinence by establishing the fitness of the problem to be handled in terms of the techniques available is to put the cart before the horse. Instruments are one thing; the calibre of the problems posed is quite another, and if we are to be the slave of our instruments we may never get fundamental problems solved or we become involved in minutiae which we hope will add up at some millennial date. We reach a kind of statistical metaphysics where the nature of the problem has been shrouded in the obscurity of the symbols by which alone it is permitted to be prosecuted.

This transformation of social science from a field of knowledge into a bundle of research-techniques is strictly in conformity with the structuring of behavior along procedural lines in all parts of American culture. The way you go about things becomes far more important than what you go about. A new occupational group—administrators and administrative-researchers—has grown up, who for a consideration will find out for all people how they can accomplish whatever it is they wish to accomplish. The aim is to turn out an organized professional group of public-opinion pollers, market-researchers, interviewers, and enumerators, who will aid men to solve the problems of the culture within the ambit of the mechanisms existent. It is highly noteworthy that the kind of problems dealt with by this school of thought is considered quite innocuous by the administratively and economically affluent. Can it be that these techniques are inherently bulwarks of a *status quo*? Or is it that addiction to this point of view is itself a social ideology, having as its concealed major premise that any society that will let social research be carried on in this

fashion is worth defending as already being properly organized? Lundberg is much encouraged because physical scientists, large corporations, and other groups are becoming enamored of the uses of social science.¹⁴ The more they look with favor upon the kind of problems this approach deals with, the more satisfied Lundberg is that all is going well with social science. It has not yet struck him that these groups are enraptured with his viewpoint and the sort of problems it can handle, precisely because it is so narrow that it asks only questions they want answered—or asked.

Lundberg is very much concerned about the misguiding of "students into historical and philosophical blind alleys instead of providing them with the technical equipment upon which all modern science relies for verification of its theories."¹⁵ These blind alleys can, however, never be as blind as the insistence that our society be construed as structured in such a way as to be capable of statistical manipulation by handling only such data as conform to the canons of such manipulation. One does not gain comprehension by torturing data into preconceived symbolism. That social research does honor to this approach is less a reflection upon it than upon the culture which allows it to flourish as the finest growth of scientific achievement. Our culture in the higher sociological learning has become a technicians' utopia, and thus comes to reflect the high technological achievements of our society despite the low estate of our ability to handle the human adjustments to this technology in terms of the democratic ideals professed.

4. *Imputation of motives.* On this basic problem, Lundberg tells us in one book that "human motives are anything but immune to scientific investigation."¹⁶ In another book, he writes: "We no longer find it helpful to bother with the motives of tornadoes, bacteria, or even of the higher animals, except man. . . . In the social sciences it is felt we must go 'farther'. . . . It does not follow that we should engage in a futile search in nature for entities to correspond to these animistic words that happen to be part of our vocabulary. When the interaction of the observable components of a situation has been described scientifically, purpose and motive have also been described, and all scientific

¹³ *Foundations of Sociology*, p. 122-3.

¹⁴ *Can Science Save Us?*, p. 55.

¹⁵ *Foundations of Sociology*, p. 123.

¹⁶ *Can Science Save Us?*, p. 19.

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'purposes' have been served."¹⁷ According to Lundberg men impute motives to events according to their predilections. "The same event may be attributed to economic motives, the Oedipus complex, or the conjunction of the planets, according to whether one is an economic determinist, a Freudian, or an astrologer."¹⁸

For psychoanalysis Lundberg appears to have little use until its terminology can be translated into terms of behavioristic and operational psychology.¹⁹ "To a scientist, the motives of a stone rolling downhill or of a boy murdering his father are simply the full set of circumstances resulting in either event. These conditions are equally subject to scientific investigation in both cases."²⁰ He accepts the "cures or satisfaction afforded such [psychoanalytic] patients and practitioners of psychoanalysis" and does not question them "any more than I question the therapeutic value of the Catholic confessional, Christian Science, or the Grotto at Lourdes."²¹ He believes that psychoanalytic interpretation "consists apparently of injecting 'motives' and 'goals' into the situation. From my point of view, motives and goals can be inferred only from behavior."²²

Lundberg's remarks on psychoanalysis are most revealing of his stand here. He is steadfast in his refusal to acknowledge that behavior takes place on an "unconscious" level and that overt behavior is not necessarily an index of what is happening to make the individual act the way he does. With such concepts as frustration, anxiety, rationalization, projection, ambivalence, and catharsis he will have no traffic, since he cannot subsume them under what he thinks are conventionally accepted scientific methods. He has decided what he is going to permit to be called scientific, and it does not seem to matter what we have learned about the depths of human motivation and the structure of personality after half a century of clinical investigation. Whatever does not fit his impoverished categories of "attraction-repulsion," "interaction," and "co-operation, competition, conflict," is banished from social science. By shrouding relationships in a new behavioristic jargon he is sure he will be able

to attack problems which he does not permit himself to recognize. And Lundberg has on his side the fact that he has already made it clear that it will take years, perhaps centuries, before all of these behavioristic concepts will be sufficiently investigated in all their presumed richness and variety. Human emotions which go deep into the life-history of the individual are reckoned away by stimulus-response, attraction-repulsion, association-dissociation, and other superficial terminology. Sociology and social psychology become sciences by thinking away all problems and approaches which do not fit into the preconceived methodology, which has been uncritically borrowed from other sciences. Lundberg (and it must be remembered that he is here throughout considered typical of many others) appears ready to charge that psychoanalysis invented what it perhaps unfortunately calls the Oedipus-motive. He seems unwilling to admit that it was first forced upon Freud by the incidence of cases. He appears to be implying that these fundamental principles of human motive-analysis were somehow guessed at or fabricated out of whole cloth. It may be acknowledged that psychoanalysis has been lax in making statistical computations of its cases without thereby invalidating its principles of interpretation of behavior.

5. *Values, social science, and the "good society."* On values and valuation, Lundberg writes: "Since valuations or values are empirically observable patterns of behavior, they may be studied as such, by the same general techniques we use to study other behavior."²³ And "the only value judgments which any properly trained scientist makes about his data are judgments regarding their relevance to his problem, the weight to be assigned to each aspect, and the general interpretation to be made of the observed events."²⁴ Science, he says, cannot be identified with any social program, religious sect, or political party. He writes: "The favorite cliché is that 'science can flourish only in freedom.' It is a beautiful phrase, but unfortunately it flagrantly begs the question. The question is, under what conditions will the kind of freedom science needs be provided?"²⁵ And he concludes that "the services of real social scientists would be as indispensable to Fascists as to Communists and Democrats, just as are the services of physicists and physi-

¹⁷ *Foundations of Sociology*, p. 222.

¹⁸ *Can Science Save Us?*, p. 19.

¹⁹ *Foundations of Sociology*, p. 285.

²⁰ *Can Science Save Us?*, p. 19.

²¹ *Foundations of Sociology*, p. 284.

²² *Op. cit.*, p. 286.

²³ *Can Science Save Us?*, p. 26.

²⁴ *Op. cit.*, p. 28-9.

²⁵ *Op. cit.*, p. 45.

cians."²⁶ As for himself, he is a democrat, but he writes: "My attachment to democracy may be, in fact, of *scientific* significance chiefly as indicating my unfitness to live in a changing world."²⁷ He is so partial to democracy "with all its absurdities, that I would find some current alternatives quite intolerable, and that I may even find it worth-while to go to any length in defense of democracy of the type to which I am accustomed."²⁸

"In short," Lundberg concludes, "we merely obey here the ancient injunction to render unto Caesar the things that belong to Caesar. The more general form of that adage is this: Render unto science the things that belong to science and to metaphysics the things that belong to metaphysics."²⁹ Finally, "it comes down, then, to this: Shall we put our faith in science or in something else?"³⁰ And, "If we do not place our faith in social science, to what shall we look for social salvation?"³¹

Lundberg appears to be eminently correct in denying that the social scientist must inject his own values into a study of a social problem, whether in a primitive society or in modern American culture. And the scientist, as citizen, Lundberg claims, has no "pull" with the higher powers which gives him the authority to tell men what to do. But he moves from this position to one where he finds that there is no social environment in which social science flourishes more than in any other. Science may be the handmaiden of Communism, Fascism, democracy, technocracy, or what have you?

But science is also a social phenomenon, and it carries values with it, despite Lundberg's protestations: that is, the assurance of opportunity to discover the truth whatever it is, the assurance that men are permitted to lead the life of reason and science, that opportunity for individual development be open to all on the proven ground that intellectual ability is found spread throughout the population regardless of family, birth, race, creed, and color. For Lundberg, a *real* physician would have no compunction about being employed in Buchenwald and a *real* physicist would see nothing wrong with banning Jews from physical science. And a *real* social scientist would have no compunction

about working for Herr Goebbels on how to conceal from the people the actual basis of political power behind a *mélange* of lies and appeals to brutality. And everybody knows that the Russians permit free and open investigation into the effect of governmental bureaucracy upon the freedom of learning and the structuring of personality.

If social scientists ask only the questions that those in power permit them to ask, then of course Lundberg is correct. But the problems which social science faces are not set, scientifically, by what those in power want answered or will permit to be answered. Does Lundberg think that it makes no difference to social scientists what type of society they live in, since the kind of questions they ask and answer can serve to make them *personae gratae* under any system? He writes: "I have emphasized that physical scientists are indispensable to any political regime. Social scientists might well work toward a corresponding status. Already some of them have achieved it to a degree. Qualified social statisticians have not been and will not be disturbed greatly in their function by any political party as long as they confine themselves to their specialty. Their skill consists in the ability to draw relatively valid, unbiased, and demonstrable conclusions from societal data. That technique is the same, regardless of social objectives. No regime can get along without this technology."³² But technology is a servant of the kind of hypotheses that are being prosecuted, and of course if no challenging hypotheses—calling into question the ability of social structures to achieve aims agreed upon—are set up, nobody would wish to disturb the sleeping dogs. This, to mix the metaphor, is a new, streamlined version of the ivory-tower.

It is revealing to note under what auspices Lundberg expects social science to advance. "Shall we put our faith in science or in something else? . . . This is the question which ultimately must be answered by everyone, but first by scientists themselves, by legislatures, by the Foundations, and by individuals who endow and finance research and education. If it is answered in the affirmative, then social research institutions will make their appearance, which will rank with Massachusetts and California Institutes of Technology, Mellon Institute, the research laboratories of Bell Telephone, General Electric and General Motors, not to mention some two thousand others. For some time the

²⁶ *Op. cit.*, p. 48.

²⁷ *Op. cit.*, p. 46.

²⁸ *Op. cit.*, p. 46.

²⁹ *Op. cit.*, p. 97.

³⁰ *Op. cit.*, p. 114.

³¹ *Op. cit.*, p. 104.

³² *Op. cit.*, p. 48.

sponsors of these enterprises devoted to physical research have been wondering if the solution to social problems does not lie in the same direction. When they undertake to support social research as generously as they have supported physical research, they will obtain comparable results."²³

In truth, Lundberg does not seem to understand science as an intellectual pursuit, and certainly not as the highest achievement of human reason, particularly since human reason turns out to be a form of adjustment, just like any other. His view of science suffers from compounded ambiguity. He does not understand that science can be considered in three frames of reference: (1) the accumulated knowledge we have of segments of the universe, natural, human, and social; (2) the only method of arriving at true propositions; and (3) a collective enterprise entered upon by groups of individuals. As the third it carries no ethics with it for Lundberg. But surely he cannot fail to admit that science is the sworn enemy of human prejudice, intolerance, superstition, and all other methods alternative to the discovery of truth. Science as a sociological phenomenon is a morality; as well as being a method of inquiry as a logical and methodological phenomenon, and a statement of our accumulated knowledge as an epistemological phenomenon.

Lundberg's approach gives all groups in society the right to claim scientific sanction for their behavior in the name of the instrumentality of science. Anybody can be shown how to achieve anything; and instead of a code of ethics strictly derivative from the practice of the calling of a scientist, we have a lavishly elaborated anarchy of tastes and viewpoints all capable of operating with the findings of science even if they violate all the canons of its ethics. This serves as a very handy apologia for all kinds of useless research being done today all over the country, and it places the sociologist on such a sanctimonious, impersonal level that his every conclusion is above suspicion of being prejudiced or ill-intentioned. The sociologist appears as a disinterested spectator from Mars, able to point out to men their foibles and follies, himself above the strife and ready to serve all confused human beings of whatever race, creed, and previous and present condition of servitude—or political belief.

Lundberg fails to appreciate what scientific endeavors do to men. For men to accept scientific

solutions to problems posed by the social sciences requires that men have learned to set store by rationality, that "appeals to reason" will be needed. In turn, of course, it means that society be so structured that men of science can investigate social problems without fear of persecution, and that there be some power-instrumentality whereby they may work for the application of such solutions. But this involves the social scientist in (1) a set of values as to what kind of society this is; and (2) the necessity for defending that society as the good society. Such a society is what we mean by democracy. Democracy is a structuring of society so as to achieve and apply, as far as possible, the rational findings of social science to the problems posed to men. It is a process of achieving a rational society, and the instrument of achievement is science. Wherever the findings of science are not applied, democracy is retarded. To the degree that men can nevertheless urge such solution, to that degree democracy is still possible.

Highly questionable is Lundberg's view that social science is in a very undeveloped state. It is, however, most amazing how much we know about the structure and functioning of social relationships in different cultures, including our own. What is more amazing is how little we do with our knowledge. It is puzzling that he, at this stage of scientific knowledge, should contend that the reason the findings of social science are not listened to is that we know so little scientifically about social problems. The findings of social science are not heeded because society is not rationally structured. That society is not rationally structured is demonstrated when social scientists think that the way to advance social science is to surrender its birth-right as the analyst and critic of social structures and to gather unto themselves little jobs for corporations, fund-raising associations, magazines interested in market-research, and other oddments of American culture. This hired-man status of the social scientist itself reflects the non-rational values of our culture.

Reason involves an acceptance of those values inherent in science as a social calling—of tolerance, equality of opportunity, freedom of discussion and press and assembly, and of co-operation. The divorce of science and values is a divorce of science as the method of arriving at, and as a corpus of, valid propositions, and science as an activity which involves a structuring of social relationships and an interdependence of function in a joint, collective under-

²³ *Op. cit.*, p. 114-5.

taking. In the former, science is seen as a study of objects; in the latter, it is seen as the unity of subject and object. To assume that scientific self-consciousness (man's consciousness of himself as rational or capable of rationality in spheres where he has not yet reached valid propositions) and science as a body of knowledge and a method of inquiry are bifurcated is to fall foul of the pluralistic fallacy that since man participates in our society in many groups therefore he himself is not a unity. It is the divorce of reason from its highest manifestation. It is bad enough when natural scientists come to believe that there is no necessary connection between their vocations and their values; but here comes a social scientist to announce to the natural scientists that they are right in assuming this bifurcation, and that man lives many lives in a pluralistic society. This invitation to irrationality is of course very seductive since it requires that reason not be pushed too far.

It is altogether likely that the position represented by Lundberg will result not in the furthering of social science, but in revealing to men how muddled and confused, how deceptive and laden with false promises, and how far removed from their basic problems is this approach which has captured large segments of American social science by technical *coup d'état* and by selling its wares to all comers. It is to be hoped that being all things to all men is only a temporary avocation of social science in America.

THE TREATMENT OF UNIONISM IN "MANAGERIAL SOCIOLOGY"*

HAROLD L. SHEPPARD
Wayne University

The purpose of this article is to evaluate the writings of Elton Mayo and his followers insofar as these writings pertain to the subject of unions. This attempt is of legitimate interest to sociologists because of the great influence which Mayo has exerted on the development of industrial sociology, despite the fact that some would not consider him to be a sociologist.

The term, "managerial sociology," is used here to designate that type of research and analysis in industrial sociology which is carried on within a framework of values possessing a

* Adapted from a paper delivered at the meeting of the Michigan Sociological Society, Ypsilanti, Michigan, November 19, 1948.

managerial orientation.¹ Furthermore, the following brief remarks on the specific subject of unionism must be placed against the backdrop of a perspective, or ideology, characterizing the literature of the Mayo group. It is possible to trace this perspective from as early as 1919 when Mayo wrote *Democracy and Freedom* in Australia, to 1947, when his political *Problem of Industrial Civilization* appeared. It is necessary to refer to the presence of this socio-political structure of thought because of the current impression in many quarters that the famous Hawthorne Experiments alone provide the indisputable, empirical support for the Mayo group's pronouncements on various industrial relations issues, for example, unions.

T. N. Whitehead, in his *Leadership in a Free Society*, has provided us with one of the group's rare formal statements on unions. In the chapter entitled "The Function of Trade Unions" he gives us insight into what the Mayo group takes to be the essential nature of unions. Whitehead writes that in the pre-industrial society, or *established society* as Mayo expresses it in his own glowing descriptions of that Golden Age, the relations between master and men were personal, the men had security, and they had a "human function to perform in the social structure." The organization of unions took place, Whitehead claims, because workers in industrial society found themselves "cut off from the rest of society." This type of interpretation is misleading, if not confusing and question-begging. Certainly the classic study by John R. Commons of the shoemakers revealed no breakdown of the personal relations between master and men as the cause of the formation of the shoemakers' union. In fact, the opposite is closer to the truth: a shift in such relations developed after the shoemaker journeymen organized into unions because of the weakening of their bargaining position brought about by the expansion of the market.²

Another characteristic of the Mayo group's structure of thought is revealed in the following statement, again from Whitehead:

It is interesting to notice that many of the

¹ For a detailed discussion, cf. the author's unpublished doctoral dissertation, "Managerial Sociology: A Critical Commentary on the Mayo Influence in Industrial Sociology," University of Wisconsin, 1948.

² *Labor and Administration* (New York: Macmillan Co., 1913), Ch. XIV.

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early workers' unions, especially but not entirely outside England, did not primarily aim to improve the material conditions of the worker. The movement was directed towards establishing a new ordering of society. This new order, which with variations was always socialistic, would give to the workers the honourable task of leadership and organization within society . . . it was a measure of the degree in which the proletariat had lost touch with their communities, that the loyalties or interest which the new order was to promote were not those current at that date. (*Leadership . . .*, p. 143)

This statement is typical of the Mayo group's tendency to minimize the importance of "material conditions of the worker" as a factor in unionism, and the tendency to "platonicize" the term "community"—the workers being considered as outside this entity whenever they move towards unionism and/or socialism. This view is strikingly similar to a favorite theme in anti-union newspaper editorials, in which "unions" are pitted against "The Public." It is also similar to what C. Wright Mills has cited as a feature of the professional ideology of social pathologists.³

We come now to a further feature of the Harvard group's views on unionism. "The most impressive function of trade unions," according to Whitehead, "has been in the social activity they have promoted." The meaning of "social" here must not be confused:

In a country where labour is so highly unionized and where union activity is so great, it is natural that unions should have functioned in some ways as social clubs for their members, and they have taken their part in the organization of social gatherings in the narrow sense of the term. (*Leadership*, p. 146)

Throughout the literature of the Harvard group the term "social" is used in much the same way. In no way does it include problems in classes, of distribution of political and economic power, conflicting ideologies, etc. The "social" function of a formal organization like a union is its primary function, according to this perspective of managerial sociology. The informal, seemingly incidental, aspects are viewed as the core function. Thus the derivative, incidental aspects of unions are blown up into basic cause, function, and purpose of unions.

Obviously, if unions are to be considered as

social clubs for the workers in compensation for their loss of personal contacts with the employer and manager, then the economic and political aspects are excluded or minimized. This is in line with another feature of managerial sociology, namely, a tendency to take a great deal of pleasure in lengthy assassinations of the straw economic man.

For Whitehead, company unions are viewed as a result of the weakness of unionism. Here again, the interpretation is slightly confusing. One might equally argue that the weakness of independent unionism is a result of company unions. In all this, it should be kept in mind that Whitehead's remarks were written before 1936, before the immense expansion of independent unionism in the last decade. However, he took no pains to revise his views in the second edition of *Leadership in a Free Society*, published in 1947. A company union, according to him, "is an association of workers of a given company, with officers who represent their wishes and attitudes to the management." That such a union's bargaining power is pitifully weak, Whitehead does not deny, but he goes on to assert that, "Where the relations between management and employees are those of collaboration based on mutual trust, a company union has proved a flexible instrument and one capable of being adjusted to the real [?] situation, and it has shown great possibilities under such circumstances."

Such a statement and others by members of the Mayo group reflect the utopian conditions which form part of the unstated normative structure of thought underlying the group's discussion of industrial relations issues: separate (insofar as employee organization is concerned), unaffiliated organizations of each plant's workers collaborating in "mutual trust" with management. This suggests a social psychology of "plant-consciousness," as contrasted with "class-consciousness" and "job-consciousness." Company unions are justified "as an attempt to integrate the various interests involved."

In this connection, the relationship between managerial sociology's naive and oversimplified treatment of interests on the one hand, and its emphasis on "social skills" and "communication" on the other, should be mentioned. The common argument in this context, that industrial peace depends upon the exercise of "social skills"—of tact and forbearance—rests on the assumption that a fundamental identity of interests exists between the parties involved

³ "The Professional Ideology of Social Pathologists," *American Journal of Sociology*, September, 1943.

in industrial relations, and that this identity is increasingly interrupted by regrettable misunderstanding or breakdowns in communication. This is no new assumption. What is new is the terminology, borrowed in large part from sociology and anthropology. Commenting on the idea nearly twenty years ago, Richard Tawney wrote that, "The disputes which matter are not caused by a misunderstanding of identity of interests, but by a better understanding of diversity of interests."⁴

The remainder of Whitehead's chapter on unionism consists of a gentle bit of advice to American management. He warns that the future of American unionism depends "on the degree to which social living is made a first concern of those who are in a position to lead." As a result of the slackening of social mobility in America, he gloomily prophesies, "a self-conscious class of manual workers is growing who will not think of themselves or their children as likely to occupy any other position." And this would result in an increase in the strength of unions. This is regrettable because:

In spite of many exceptions [how many exceptions are necessary before one may reject a generalization?—HLS] These unions are not adequately led and have no great tradition of collaboration with management, and the future history of human relations in industry might be an unhappy one. (*Leadership*. . . ., p. 155)

It would be interesting to go into the implied connotation here that the source of failure of industrial peace lies in the type of union leadership but, in a way, it is touched upon when one asks the following question: If union leaders are not adequate, then who are?

The unabashed answer given by Whitehead, and seconded by Mayo and Roethlisberger in their writings, is that the best brains of the country are to be found in the executive ranks of business, and that they will adapt the "organism of industry on lines more satisfactory for those involved." When this happens, unionism will be weakened since the workers will "find in the direct collaboration within the factory all they need in the way of personal self-expression and of adequate consideration." No commentary is necessary at this point, other than that Whitehead, like his colleagues, is apparently ignorant of, or chooses deliberately to neglect, the history of industrial relations

⁴ *Acquisitive Society* (New York: Harcourt, Brace & Co., 1920), p. 40.

and the reality of the present economic and political scene in America.

Another item that needs critical examination in the same fashion is Mayo's proposition that the essence of unions lies in the spontaneous tendency of workers, *within* the place of work, to form small, informal cliques. Under the idealized conditions of worker-management relations envisioned by managerial sociology, there would be no need to formalize such groups or coordinate them into autonomous organizations, i.e., unions.

Ordway Tead has criticized the Mayo group for their failure to take up the question of how, where, and why such informal groups are developing into *union* groups.⁵ He further criticizes them for the lack of consideration "of the role that may be played by labor unions in giving workers a total sense of security, self-respect, and opportunity to voice complaints and suggestions." And their systematic neglect of "collective and organized approval by and of the workers autonomously is presumably deliberate," in Tead's opinion. To him it constitutes a "serious blindspot in thinking about and in analyzing this significant problem of employee participation and co-operation."

But collective bargaining, according to Mayo, merely masks the fundamental problem involved in industrial relations, namely, the disastrous decrease in the "human capacity for spontaneous co-operation" [as opposed to organized co-operation]. Furthermore, it is virtually worthless, because at bargaining conferences the parties involved discuss *consciously* perceived grievances and issues, whereas the basic, real problems are *unconscious* ones which manifest themselves in the form of wage disputes, etc. Finally, collective bargaining is anti-social, since the presence of two opposed groups in the persons of management and union representatives only signifies and crystallizes class-conflict.

Roethlisberger, in *Management and Morale*, states that preoccupation with such matters as "the effects of collective bargaining upon wages, employment . . . and competition between union and non-union plants" is merely verbal controversy. For him, the evidence of the Hawthorne researches points to the following suggestion:

. . . have we not a clue as to the possible basis for labor unrest and disputes? Granted that these

⁵ Review of Mayo's *Social Problems of an Industrial Civilization*, in *Survey Graphic*, May, 1946.

disputes are often stated in terms of wages, hours of work and physical conditions of work, is it not possible that these demands are disguising, or in part are the symptomatic expression of, much more deeply rooted human situations which we have not as yet learned to recognize, to understand, or to control? (*Management and Morale*, p. 25)

Apparently, the grievances made so much of by workers are actually the manifest content of a situation today in which people live in a "social void" and without social function. When workers become "an integral part of the social situations in which they work, a legal contract is not of the first importance."

To be sure, there is much to the argument against seeking *ad hoc*, immediate, and superficial explanations of conflict in industrial relations, but what is questioned here is the particular approach by the Mayo school. It is too speculative and nonverifiable. In his attempt to avoid *ad hoc* explanations, Mayo goes too far away from the actual situation and builds up a *Weltanschauung* based in large part on his interpretations of Durkheim's and Pareto's sociologies, and on what might be called "anthropological romanticism."

To repeat, we do need a perspective encompassing more than the superficial causes of the present industrial struggle. Sociologists have at their disposal the Wisconsin studies which constitute a valuable foundation in the historical sociology of American industrial relations. Particularly relevant in the present context is Selig Perlman's emphasis on the social psychology of "job-scarcity consciousness":

"Scarcity groups" regularly endeavor to "own" as groups the limited opportunities at their disposal. Thus no issue relating to the conditions upon which they will permit an individual member to connect with an opportunity can escape becoming strongly tinged by this fundamental aspiration to "own" all the opportunities extant. *It would be erroneous to try to account for an industrial struggle solely by the specific demands which are its proximate causes:* wages, hours, freedom from discriminatory discharge, etc., while leaving out this group "hunger" for controlling the job opportunities to the point of "ownership." . . . behind each strike there always lurks the struggle for the control of the jobs.⁶

These remarks bring us back to Whitehead's interpretation of unionism. Opposed to his analysis is the contention that the *raison d'être*

for unionism is job security for the worker, centering about the wage issue, the rationalization of personnel policies, and the area of control by the worker or his agents over his own affairs. Neil Chamberlain's statement on this issue might well be taken as directly aimed at the type of interpretation outlined in the first part of this article:

The union is an instrument, a tool, offering satisfactions of its own, it is true, but created for the basic purpose of influencing business decisions. *It is not a social club, though it offers social pleasures.* It is not a professional society, though its membership may have common occupational interests. It is not an educational organization, though it may sponsor training programs. It is essentially an economic organ. . . .

In summary, this rather cursory glance at the treatment of unionism by the Mayoites should indicate the necessity to examine: 1) the total value framework that subtly permeates their discussions; 2) the social function of their explicit analytical framework; 3) their explanations of the origins, functions, and "essential nature" of unions; 4) the degree of their seemingly systematic neglect of the existence of unions and of the role they do or might play in given spheres of industrial relations in particular and in socio-economic ones in general.

COMMUNICATIONS AND OPINION

COMMENTS ON RELATIONSHIPS BETWEEN RESEARCH AND THEORY

The attempt on the part of Nancy K. Jack and Betty Schiffer in their paper "The Limits of Fashion Control," appearing in the December, 1949 issue of the *Review*, to integrate their empirical research into the larger framework of social theory is commendable. The procedure is suggestive of the comments by Robert Merton concerning the relationship between research and theory appearing in the October, 1948 *Review*.

The consequence of a great deal of research has been negated by a failure to set the research into a significant framework of general theory. The application demonstrated by the authors of "The Limits of Fashion Control" is all to the good. However, there is another facet to the

⁶ *The Union Challenge to Management Control* (New York: Macmillan Co., 1928), pp. 242-43. Italic mine.

problem which restricts the significance of research besides relating specific research to larger and more general frames of reference. This area is concerned with the matter of focus of the research and its level of significance.

Actually, how important is it in the area of social control that the "woman-on-the-street" did not follow the exact dictates of the fashion creators; that there are limits to the extent of dress length in this conformity pattern? Let us, instead, focus on the relationship demonstrated between the dictates of those who set fashion and the follower pattern of the "woman-on-the-street." A rather consistent, although not exact, pattern of conformity is demonstrated. Now, from the point of view of the fashion dictator, how much consistency is necessary to fulfill the purposes of the dictation? This depends upon the purpose.

In terms of the vested interest of the designer and the allied garment manufacturer, mere change in dress length and design, even though minute, may prove ample for their purposes in the attempt to institute change in fashion. Here, then, focus upon the purpose and fact of change in dress length (once there is a demonstrable relationship between fashion dictation and conformity by the population) becomes of major significance; the discovery that the explicit design dictated was not followed in detail is minor.

The conclusion that the fashion dictator is bound within certain limits by the receptiveness of his subscribers is of minor importance if the functions of his dictations are fulfilled. No doubt there is a limit to which dictation of any sort can be carried. However, extremes of control are seldom necessary for the fulfillment of the purposes of the dictation, in a working situation. This is true in market relationships, in the fields of fashion and advertising, and in power operations utilizing propaganda and legal control mentioned in the article.

It is not the design of these comments to make this article a whipping boy, but rather to point up the necessity of discerning and utilizing significant clues in research and setting up the research so that it will actually contribute purposeful data in a larger frame of reference. The comments further suggest that the findings of any research can lead on in many different pathways; it is important in terms of conservation of energy and resources to follow those most fruitful in terms of their level of significance.

RICHARD M. STEPHENSON

New Jersey College for Women

NEWS AND ANNOUNCEMENTS

The American Anthropologist. Announcement is made of the new editorial board, beginning with the January issue.

Editor: Melville J. Herskovits, Department of Anthropology, Northwestern University, Evanston, Illinois.

Associate Editors: Sol Tax, Department of Anthropology, The University of Chicago, Chicago, Illinois; C. W. M. Hart, Sterling Hall, Department of Sociology and Anthropology, University of Wisconsin, Madison, Wisconsin; W. C. McKern, Public Museum, Milwaukee, Wisconsin.

Book Review Editor: Alexander Spoehr, Chicago Natural History Museum, Chicago, Illinois.

The American Library Association has just published a *Bibliography of Place Name Literature* which may be of considerable interest to those working in the fields of geography, history, and some of the other social sciences. It covers the United States, Canada, Alaska, and Newfoundland and helps in the location of books, magazine articles and some manuscripts, dealing with the origins, meanings, spellings, and pronunciations of place names, place nicknames, mountains, regions, rivers, etc. A detailed index makes it possible to locate information on various types of names as those of Indian or foreign language origin. For those interested in research in this general field the book shows up many gaps to be filled. It was compiled by librarians Richard B. Sealock and Pauline A. Seely.

Current Digest of the Soviet Press. To make information regarding the Soviet Union more readily available, the Joint Committee on Slavic Studies, appointed by the American Council of Learned Societies and the Social Science Research Council, has undertaken to publish the *Current Digest of the Soviet Press*.

The *Current Digest* began regular publication as a weekly in January, 1949. Each week it presents a selection of the contents of the Soviet press, carefully translated into English, objectively condensed by competent editors, arranged by subject matter, and indexed quarterly. The translations are presented as documentary materials, without elaboration or comment.

All the material in each issue is grouped under subject-matter headings, and subscribers will receive a quarterly index to the *Current Digest*. The index will also cover the contents of Soviet publications printed in English for foreign consumption. It is planned to include listings of plays performed on the Moscow stage (with number of performances), and listings of public lectures announced in the Soviet press.

The subscription rate for the *Current Digest*, including the quarterly index, is \$150.00 per year; single copies, \$3.00. Universities, colleges, libraries, research centers and scientific or educational bodies

subscribing to the *Current Digest* may obtain supplemental individual subscriptions for their own use or for faculty members, staff members, or associates, at a special rate of \$25.00 per year.

All communications should be addressed to the *Current Digest of the Soviet Press*, 1219 Sixteenth Street, N.W., Washington 6, D.C.

Russell Sage Foundation. According to its Annual Report 1947-48, the Board of Trustees has adopted several new policies, the most fundamental of which looks toward a closer and more effective relationship between social welfare practice and the social sciences. "As the Foundation sees the situation, both social science and social practice have made great advances in recent decades, but as they have progressed there has been costly failure by each to maintain sufficiently close liaison with the other. Research needs to be kept realistic by contact with the practitioners who use its results; the practitioners need to keep informed about the frontiers of research knowledge bearing on their techniques." The Foundation has begun to explore opportunities for aiding in the development of improved liaison between research and practice in the broad field of human welfare.

District of Columbia Sociological Society. The January meeting was devoted to a discussion of World Population and World Resources, with Dr. William Vogt, author of *The Road to Survival*, as the main speaker and with a panel consisting of Dr. O. E. Baker, Head of the Department of Geography of the University of Maryland; Dr. Conrad Taeuber, Head of the Division of Statistical Standards, FAO; Dr. Robert C. Jones, Chief of the Division of Labor and Social Information of the Pan American Union; and Dr. Horace Belshaw, Director of the Rural Welfare Division, FAO. The Society is now publishing a Newsletter to report the developments in its area.

Centre College. The Sociology Department of Centre College in association with civic organizations in Danville will sponsor a two-day Conference on Juvenile Delinquency on June 14-15, 1949. At that time Judge Harold M. Kennedy of Brooklyn, New York, and Judge Camille Kelley of Memphis, Tennessee, will be presented to audiences from the Bluegrass region of Kentucky. State leaders from Louisville and Lexington will help to round out the professional sections of the program. The Conference is being prepared especially for public school personnel, social workers, ministers, parents and others whose work brings them in contact with teen agers.

Dr. J. T. Richardson of the Department of Sociology has received a Carnegie Research Grant to permit further study on his forthcoming book *Medicine in Modern Society*, this summer. Library facilities of the New York Academy of Medicine will be used for the final phases of the research.

Dr. Richardson will return to Centre College for the opening of the fall semester.

Columbia University. Paul F. Lazarsfeld is on sabbatical leave during the academic year 1948-49. During the summer of 1948, he was Walker-Ames professor in the department of sociology at the University of Washington. As visiting professor at the University of Oslo, during the winter session, he helped initiate a division of social research in that Norwegian university. He is presently visiting professor in the Department of Social Relations, Harvard University.

Robert M. MacIver has accepted a visiting professorship at the London School of Economics during the winter session of 1949-50.

Professor Kingsley Davis, formerly of Princeton University, has accepted appointment as Associate Professor in the Department of Sociology and Associate Director of the Columbia Bureau of Applied Social Research. In connection with his affiliation, a Division of Population Research has been established in the Bureau with him as Director.

Conrad Arensberg, Associate Professor of Sociology at Barnard College, is serving as Editor of *Applied Anthropology*.

Robert K. Merton is Acting Director of the Bureau of Applied Social Research during the period of Professor Lazarsfeld's leave of absence.

Dr. Leo Srole, formerly Welfare Director of UNRRA Displaced Persons Operation, has been appointed to the Division of Mass Communications research in the Bureau.

Dr. Marie Jahoda, formerly with the Scientific Department of the American Jewish Committee, has been appointed to the Division of Urban Research (Community and Housing) in the Bureau.

The Bureau has developed the following programs of specialized training in research and research application:

(a) Ten students drawn from 9 Latin American countries have had a two-month program involving fieldwork and research analysis. This was under the sponsorship and auspices of the U. S. Bureau of the Census.

(b) Six radio program directors drawn from the American, French and British zones of Germany were given a six-month training program in the techniques, practices and research methods of American radio. This was under a grant from the Rockefeller Foundation.

(c) Nineteen students from 12 Latin American countries took part in a two-month training program in research methods, with the co-operation and under the auspices of the U. S. Bureau of the Census.

(d) It is also planned to have six radio program directors from Japan and four from Korea given a training program comparable to that provided for the German broadcaster group.

Paul Lazarsfeld and Frank Stanton are co-editors of the forthcoming volume, *Mass Com-*

munications, 1948-49, to be published in the Bureau series by Harper and Brothers.

Paul Lazarsfeld and Patricia Kendall have completed an analysis of *Radio Listening in America*, based on fieldwork material collected by the NORC. The book will be published by Prentice-Hall.

Under a grant from the Puerto Rican government, the Bureau has completed a study of the Puerto Rican migrant to New York City. This research was conducted by C. Wright Mills as director, Clarence Senior, recently head of the Research Center at the University of Puerto Rico, as associate director, and Rose Kohn, formerly of Yale University, as assistant director. It will appear in the Bureau series of publications as *The Puerto Rican Journey*.

Harvard University. In February, 1949, Harvard University established a *Research Center in Altruistic Integration and Creativity*, with P. A. Sorokin as its director. The main research tasks of the Center are outlined in Sorokin's *Reconstruction of Humanity*. P. A. Sorokin asks all social scientists and scholars who are studying one of the problems of a true socialization, altruization, and reduction of interindividual and intergroup conflicts; scientific, moral and aesthetic creativity; especially who are studying and inventing the efficient techniques for altruistic transmutation of human behavior and for mental and moral re-integration of persons and groups; to inform him about their research in exchange for information of the research of the Center. Some of the research-projects carried on outside of the Center, that are strategic for the research problems of the Center and that need a modest financial assistance may be assisted by the Center. In this way the Center aims to become a sort of a co-operative Research Center in this field. Address of the Center: Emerson Hall, Harvard University, Cambridge, Massachusetts.

Iowa State College. The staff in sociology now includes the following members: Ray E. Wakeley, in charge, Reuben Hill, Joseph B. Gittler, Walter A. Lunden, Robert A. Rohwer, Roscoe Giffin, Hugh W. Ghormley, W. H. Stacy, Don Fessler, Joe M. Bohlen, Lowell Dunigan, George Beal and Robert Schmidt.

Two honors have been recently received by Dr. Hill. He has been elected as a fellow in the Society for Research and Child Development and admitted to active membership in the American Association of Marriage Counselors.

Dr. Gittler has recently completed a study on the social orientation of the industrial worker in collaboration with Dr. Anne Gertrude Douglas of the University of Buffalo.

Two members of the staff have received advanced degrees recently. Robert Rohwer was granted a Ph.D. from the University of Wisconsin in August with a major in Rural Sociology. Joe

M. Bohlen received his M.S. in December from Iowa State College with a major in Rural Sociology.

Dr. Hill has been granted a three-month's leave of absence for this summer. He will teach the summer term at Columbia University and will then participate in a two-week's workshop on Family Relations at the West Virginia University. His manuscript on "Families Under Stress" has been accepted for publication by Harper and Brothers.

Dr. Giffin is chairman of the state committee to develop plans for the Friends Service Committee institutional units which will operate in mental hospitals and schools for feeble minded in Iowa this summer. He was director of the units operating in the summer of 1948.

Research projects now under way at Iowa State College are emphasizing the following areas:

1. Family life. As part of a five-year program of research in the dynamics of rural family living two projects are now under way; 1) demographic analysis of Iowa family behavior, and 2) a survey of the status of research on the rural family.

2. Rural organization. Dr. Wakeley and Paul Jehlik, of the B.A.E. have completed their study on rural organizations in Hamilton County, Iowa. This is one of the series of 24 county studies of rural organization being done co-operatively by the Division of Farm Population and Rural Life of the B.A.E. and various land grant colleges.

3. Minority group relations. Group prejudices in rural areas are being studied by Dr. Gittler and graduate student Shirley Schwett.

4. Juvenile Delinquency in Iowa. Dr. Lunden is making a study on juvenile delinquency related to place of residence. Commitments and rates are being analyzed for five classes of counties, varying from entirely rural to highly urban.

5. Farmer co-operatives. Don Fessler and George Beal are now analyzing data that relate to farmer attitudes toward co-operatives and co-operative policies, and the effects of co-operatives on community and family life.

Linfield College. At the beginning of this academic year Theodore B. Johannis, Jr. was added to the department of sociology as assistant professor. At the end of the summer session he received the Master's degree from the State College of Washington. His coming made it possible to introduce courses in Marriage and Family Relationships, Social Movements, Introduction to Social Work, and to expand the course in Rural Sociology.

William C. Smith continues as head of the department.

Mississippi State College. A Division of Sociology and Rural Life integrating the resident teaching, experiment station research and extension teaching in one administrative unit has recently been created. Harold F. Kaufman, Thomas L. Bailey Professor, has been appointed head of the new Division. This is the third administrative recogni-

tion of the sociology program within the past two years; a teaching department was created in 1947, and a research and extension program inaugurated in 1948.

Five sociologists form the staff of the new Division. All have some teaching; three are also engaged in research and one in extension activity.

New Mexico Highlands University (Las Vegas). Offerings in the social sciences have been expanded during the spring quarter by the appointments of Robert H. Lister, formerly of the University of Colorado, as Assistant Professor of Anthropology and of W. A. Watrous, formerly of Idaho State College, as Professor of Economics. Lister has been engaged for a number of years in pre-Columbian research in the American Southwest, Mexico, and Central America. Watrous has specialized in Russo-German commercial relations. Lister will soon complete a doctorate at Harvard, Watrous at the University of Chicago.

Dr. John Burma, Professor of Sociology at Grinnell College, will again be a visiting professor at Highlands this summer.

Roscoe Baker, Associate Professor of Political Science, and James E. McKeown, Assistant Professor of Sociology, have been conducting in-service training institutes for New Mexico state and local government employees during the winter and spring quarters.

Northwestern University. Paul K. Hatt of the Department of Economics and Social Institutions of Princeton has been appointed Professor of Sociology, effective September 1, 1949. He will specialize in demography, urban sociology and social mobility.

The 1940 summer session will offer a well rounded group of courses. Professor William L. Kolb of Tulane University will be visiting Associate Professor and will handle courses in industrial sociology and urban sociology. Of the regular staff members, Professors E. R. Mowrer, Paul K. Hatt and William Byron will teach in the forthcoming summer session.

A completely revised edition of Kimball Young's *Sociology* has just been announced by the American Book Company to appear in the late spring.

A new coordinated one-year course, "Introduction to the Sciences of Human Behavior," has been established in the College of Liberal Arts. It will be given jointly by the Departments of Anthropology, Psychology and Sociology under the immediate supervision of Professors M. J. Herskovits, R. H. Seashore and Kimball Young. For freshman students in Liberal Arts, the aim of the course is to coordinate the essential concepts and materials normally presented in the separate introductory courses of the respective fields.

As an aid in the development of this course, the Carnegie Corporation of New York has made a grant of \$13,500 to Northwestern University for

1949-50. This grant will provide three internships, one from each of the three disciplines. These interns will be selected by invitation and among other qualifications, they must have completed a Ph.D. and had teaching and research experience.

Purdue University. Raymond Wilkie, who recently completed his Master's Degree at the University of Louisville, has been appointed Instructor in Sociology for this semester. He will be teaching sections of Introductory Sociology and Social Problems.

Several graduate students have been assigned to teach beginning courses this year. They are: Olive Bowden, Mary E. Feemster, Charles C. Jones, and Sunshine Kidd.

Tulane University. A regional conference of social scientists was held at Tulane University, March 10-11, to consider problems of integration, specialization and application in the field of social studies.

Sponsored by the Tulane graduate school, the conference was attended by approximately 50 selected delegates from colleges and universities in Louisiana, Mississippi, Alabama, Arkansas, Florida, Tennessee, Oklahoma, Georgia and Texas. Consultants were Dr. Merle Curti, professor of history, University of Wisconsin; Dr. Luther Evans, Librarian, Library of Congress; and Dr. Clyde Hart, National Opinion Research Center, University of Chicago.

University of Chicago. The Committee on Education, Training and Research in Race Relations of the University of Chicago, in co-operation with the American Council on Race Relations, is conducting an inventory of research in race relations and minority group problems, in order to make available information on current research which will be of value to persons and agencies carrying on research and also to those engaged in action programs in the field. It is planned to issue quarterly bulletins describing current and recently completed research projects. Two bulletins have already been issued, dated June 30, 1948, and December 31, 1948. The inventory bulletins carry accounts of two kinds: (1) descriptions of studies reported in answer to the inventory questionnaire and (2) abstracts of studies contained in published articles, pamphlets and books. All those who are engaged in research in racial and cultural relations are invited to write to the Committee for the inventory questionnaire, on which they can report studies already completed or in progress. The address is Committee on Education, Training and Research in Race Relations, The University of Chicago, 4901 Ellis Avenue, Chicago 15, Illinois.

University of Connecticut. The department of Sociology at the University of Connecticut has been reorganized as a Department of Sociology and

Anthropology. The staff in Sociology consists of Professors James H. Barnett (Head), J. L. Hypes and N. L. Whetten; Associate Professor W. C. McKain; Assistant Professor Otto H. Dahlike and Instructor Harry Posman. The staff in Anthropology consists of Associate Professor E. G. Burrows and Assistant Professor Charles Wisdom.

A Department of Rural Sociology has been organized in the College of Agriculture which is devoted entirely to research. The staff of this Department consists of Professor N. L. Whetten, (Head) and Associate Professor W. C. McKain.

All course work in Sociology, Rural Sociology and Anthropology is taught in the general Department of Sociology and Anthropology, which is a part of the College of Arts and Sciences.

Work in Sociology and Anthropology is taught in the several branches of the University by the following staff members: Fort Trumbull Branch, Dr. Frances Underwood, and Mr. Edward Rothstein; Hartford Branch, Miss Roxane Winburne; Waterbury Branch, Mr. Murray Keefer, Mr. Harold C. Yeager, Jr. and Mr. Erwin Rubington.

A number of graduate assistantships are available for the academic year, 1949-50. These assistantships carry a stipend of \$135.00 a month for nine months and require half-time assistance from the recipient while working towards an advanced degree.

Candidates for teaching assistantships in the Department of Sociology and Anthropology should address inquiries to Dr. James H. Barnett. Those interested in research assistantships in the Department of Rural Sociology, should send their inquiries to Dr. N. L. Whetten. Letters should be addressed to the respective department head at the University of Connecticut at Storrs, Connecticut.

Beginning with the fall of 1949 work leading to the Ph.D. degree in Rural Sociology will be offered at the University of Connecticut.

University of Michigan. Professor H. Lionel Elvin, Ruskin College, Oxford University, Oxford, England, will teach in the Summer Session this year. He will give a course entitled, "Freedom and Planning in Contemporary Britain."

Dr. R. Freed Bales of Harvard University will also teach in the Summer Session. He will give work in the field of social psychology.

The Survey Research Center is establishing several research assistantships on various projects with which the Center is currently concerned, including studies of industrial productivity and morale, economic attitudes and behavior, and public opinion and understanding regarding political issues. The Center invites applications from students qualified by training and experience. Preference will be given

to applicants with Master's degrees. Opportunity will be afforded for taking academic work on a limited basis. Stipends for these positions will vary between \$3,000 to \$4,000 for the academic year. Those interested should write the Survey Research Center, University of Michigan.

University of Minnesota. A new national magazine, *American Quarterly*, devoted to the interpretation of American life and culture, past and present, for the lay reader as well as the scholar, made its debut in March.

Published by the University of Minnesota Press for the University's program in American studies, the magazine was conceived by Professor Tremaine McDowell, chairman of the program and author of *American Studies*.

The editorial board consists of William Van O'Connor as editor, and the following associates: Merle Curti, Henry Nash Smith, Laurence Schmeckebier, Herbert Schneider, Paul S. Taylor, and Rupert Vance.

University of Pennsylvania. Professor Thorsten Sellin has been elected a member of the governing board of the new International Society of Criminology, which held its first general assembly in Paris January 5-7, 1949, attended by representatives of fourteen nations, the WHO, UNESCO, the International Police Commission, the International Prison and Penal Law Commission, and the International Association of Penal Law. The Executive Committee also appointed him as the representative of the Society before the Economic and Social Commission of the United Nations and to the Second Panamerican Congress of Criminology to be held this fall in Mexico City. He is also serving as the American delegate of the Committee organizing the Second International Congress of Criminology to be held in Paris in 1950.

University of Wisconsin. Professor Lowry Nelson of the University of Minnesota will be visiting Professor of Rural Sociology for the coming summer session. Professor Nelson will teach courses in Rural Social Trends and Rural Social Institutions.

After many years of service, Professor John H. Kolb has asked to be relieved from the duties of the chairmanship of the Department of Rural Sociology. Professor William H. Sewell has been named chairman. Professor Kolb is on leave of absence for six months serving as visiting Professor of Rural Sociology at the University of Oslo, Norway. He will resume his research and teaching duties at the University of Wisconsin at the beginning of the fall semester, 1949.

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Most of the World: The Peoples of Africa, Latin America, and the East Today. Edited by RALPH LINTON. New York: Columbia University Press, 1949. 917 pp. \$5.50.

The much-discussed proposal to cure our cultural illiteracy and provincialism has produced no more ambitious and successful monument than the twelve chapters which Professor Linton has here assembled. He has not attempted a description of the whole world, but only of its major "under-developed areas," a term which because of its appropriateness and lack of invidious stigmatization seems likely to come into general use. Most of the authors are anthropologists, with a minority from sociology and other fields. With the anthropologist's omniscience, the authors attempt to tell the whole story, but not always to the entire satisfaction of specialists in other disciplines; history and the higher manifestations of culture suffer from a comparative neglect. Still, the allotment of sixty to eighty pages to a competent observer permits a penetrating and well-rounded description of the contemporary scene. There is little sociology in the narrow sense of that uncertain word; Douglas Haring provides more of it in the case of the Japanese than any other contributor, as is practically inevitable in any discussion of a culture very different from our own and characterized for hundreds of years by an almost unbelievable refinement of social relations. We may be thankful that the formula for the comparative ethnology of civilized peoples has not hardened into an unvarying pattern, for that would have stultified Haring's fascinating pages, as well as the fresh and illuminating approach of Carleton Coon to North Africa.

None of the authors permits himself the abuse of the wisecrack or that excessive dependence on some single psychological principle of interpretation which in recent instances has done social anthropology a disservice in the eyes of the general reader. In fact, there is little high level generalization except in the brief pages of Linton's introduction.

Taking the broadest and longest possible view, Linton invites us to see things and events in



their proper perspective and not to be overly impressed by our present machines and laboratories, nor too discouraged by the European "Dark Ages," which were more than counterbalanced by what was happening in Islam and China. For him, as for Toynbee, the transcendent trend to note is that which has made "one world" a functional reality in our time, a trend with which our political organization must now catch up. We must, he insists, face the fact that the rest of the world is becoming more selective in its cultural borrowing from the West, to an extent that spells widespread cultural conflict and perhaps profound satisfaction in watching the West destroy itself in a struggle between the ideologies which mean little to those parts of the world—most of it, in population and natural resources—here studied. Can we build with them a cooperative world, rather than one based on domination? The endless perspectives of the anthropologist offer the comforting reflection that even if we fail there will be others, for human life and culture are amazingly resistant and persistent.

Longer introductory sections on natural resources, by Howard Meyerhoff, and on population, by Stephen Reed, involve some duplication with the regional chapters. Both are valuable and judicious summaries; and with their conclusions that land, climate, minerals, and people do not suffice as determinants of human progress, they point the way to the strongly cultural emphasis of the succeeding chapters.

Where so much reason for gratitude and praise exists, it may seem ungracious to remark on the omission of an occasional favorite book from the bibliographies, to note that unavoidably the task of assembling and editing so massive a book takes so much time that it is in some respects out of date when it appears, or to observe that John Gillin's title, "Mestizo America," is going to displease many Latin Americans; and there are those who will think that Raymond Kennedy's outspokenness on the Dutch in Indonesia is misleading, merely adding his prestige to a common but erroneous view of a complicated situation.

These are minor matters in the appraisal of a

scholarly and informative volume which, although rarely guilty of distinguished writing, should be required reading, yielding place only to the top priority of knowing ourselves, Europe, and the Soviet Union.

W. REX CRAWFORD

University of Pennsylvania

China: The Land and the People. By GERALD F. WINFIELD. New York: William Sloane Associates, Inc., 1948. vii, 437 pp. \$5.00.

What is wrong with China and how to go about remedying her great ills are the themes of this two-part book. Part I, entitled *The Land and the People*, highlights the evils present in the Chinese situation through a sympathetic description of the many aspects of the Chinese cultural environment. The collective picture which in part comprises summaries of material given fuller treatment in other works brings together in one volume material normally sought through 10 or 15 different sources. Even without the author's penetrating comments and interpretations, it furnishes the layman with an excellent picture of China in her present stage of culture.

For the specialist on the culture of China, a distinctive contribution is Winfield's chapter on Health in which he gives the reader an intimate and vivid comprehension of the far-reaching relationship between man and diseases in China. For instance, he points out that the Chinese farmer could not afford to stop using human fertilizer, even if he were aware of the connection between fertilizers and health, because the plant food constituents present in the excreta of the average farm family in China have a value at world market prices equal to almost 10 per cent of the average gross farm business.

Winfield discounts tenancy in China as a symptom rather than a cause of the miserable situation of the peasant masses. "The less than 15% of production used for rent (in all China) cannot be the major cause of the abject poverty that dogs 70 to 80 per cent of China's farmers." In the author's opinion, the prosecution of a relentless class war by the Communists in the highly competitive and overcrowded countryside is bound to bring more loss and misery to the people of China than the political reforms they propose can possibly pay for in real improvement. The reforms proposed by the Communists solve no basic problems.

The author links China's problems to the invention of irrigation and the development of a

vegetarian diet, for out of these factors has grown the huge population dominating the Chinese scene, its very size a prime factor in her present difficulties. If there is one central problem in this book of problems, it is that overpopulation is the main root of China's flourishing ills.

In Part II, entitled *Rebuilding China*, Winfield does what no one else has been bold enough to do on so comprehensive and grand a scale and in so specific detail: that is, to draw up an integrated plan for China's reconstruction and to outline the cost and consequences for individual items of the program. Part II, even more clearly than Part I, opens the reader's eyes to the overwhelming vastness of the difficulties faced in the solution of China's ills. The solutions to the veritable maze of problems are so inextricably interlocked that the whole thing becomes, in the author's own words, a vicious circle.

The reader is startled but fully convinced of the author's humanitarian motives when he proounds the logical necessity of refraining from removing the natural Malthusian checks to population increase for the present and next generation or two. This is necessary in order to make possible a permanently improved situation for future generations. Yet the author's program of development involving betterment of agricultural production, of communications, of creating stable political government, and of sanitation and medical facilities cannot be put into effect without automatically removing many of the checks, the removal of which would nullify the attainment of the objectives. He proposes limiting the benefits of the first stages of improvement to a selected few among the population who would at the same time be educated to the desirability of birth control. However, aside from the problems of selecting the group to be specially favored, the benefits in many cases cannot become operative without community-wide participation—such as sanitation measures—and this would mean improvement to the general populace, thus leading to the undesired stimulation of population increase.

The "feats of sociological engineering" proposed in his comprehensive program, if carried out, would find China in the year 2000 with about the proportional distribution of the population by occupation that France has today. The population (on the basis of an assumed 500 million today) would have risen to a point of stability at about 650 million, with only 40% on the land in farming as compared with some

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75% today. Almost 200 million people would be engaged in industry, including handicrafts. The present low state of China's economic situation is emphasized when it is revealed that that stupendous program proposed, if and when accomplished, would only raise the standard of living for the Chinese to a level of one-fifth that of the United States in 1929. In considering Winfield's reconstruction schedule, the reader is torn between the feeling that the author has analyzed the problem in a logical and practical manner and the feeling that in spite of the author's realism, his pessimism is still too optimistic. The magnitude of the program seems to warrant at least double the 50 years allotted by the author.

The book serves an important function in reviewing through a fresh approach the non-political problems of China which basically are far more important in retarding China's modern development than the political ones so often cited today. On the other hand, as the reconstruction program outlined seems to ignore the possibility of a changed situation that would exist should a Communist dominated government of China come into being, it appears somewhat removed from the actuality of present events.

HEROLD J. WIENS

Yale University

Under the Ancestor's Shadow: Chinese Culture and Personality. By FRANCIS L. K. HSU. New York: Columbia University Press, 1948. xiv, 317 pp. \$3.75.

The Golden Wing: A Sociological Study of Chinese Familism. By LIN YUEH-HWA. New York: Oxford University Press, 1947 [1948]. xv, 234 pp. \$4.50.

These two intimate studies of the Chinese family system add rich detail and illuminating insight to earlier investigations of this basic institution by such writers as L. K. Tao, D. H. Kulp, II, Martin Yang, Hsiao-tung Fei, and others.

Dr. Hsu carried on field work for his study in West Town, a small market town in Central Yunnan. Conditions of family organization, occupational specialization, and recreational life of the villagers are reported in satisfying detail; but the particularly valuable parts of this work appear to this reviewer to be the vivid demonstration of the way in which "death does not sever the relationships of the departed with the living, but merely changes it to a different level"

(p. 239), and the author's theory of why the prominence and prosperity of West Town families generally last not more than three generations. In this little community where worship of departed ancestors is conducted in the home, at graves, and at the family temple, there is a belief that the fortunes and happiness of the departed depend in large measure upon the behavior of the living. With respect to the appraisal of the good conduct of the living or dead, there is an interesting list derived from questioning many of the villagers concerning what are considered positive virtues, good behavior, negative qualities, and evil behavior (pp 147-148). While many of the traits which would be considered praiseworthy in an American community are emphasized, "bad" qualities—seduction, adultery, extravagance, gambling, gluttony—are not neglected. Also included in the "black" list are co-education, revolt, adoption of Communism, jealousy, and fighting between those of different religious creeds.

Dr. Hsu's theory of the decline in prominence of West Town families is based on what he terms the father and son "identification" (p. 65): "whatever the one is, the other is; whatever the one has, the other has." The husband-wife relationship and all erotic impulses are merely supplementary and subordinate to this important parent-son relationship (p. 59). This "identity" is not only with the father but extends to the entire ancestral lineage. The author points out that the scion of a rich and prominent father early assumes the airs of a rich man and the place gained by his father; he does not feel impelled to work for what he receives. When responsibilities of family and clan life are thrust upon him, he has had no training whatsoever in the handling of either money or affairs. Consequently, according to Dr. Hsu, these young men who attain status without effort generally become failures in the discharge of their duties and the family group begins to decline in prominence and prosperity until the third generation. Mitigating factors are indicated, however. In West Town, unlike most of China, there frequently occurs an early division of property within the larger household, although ceremonially and religiously the family remains intact. This brings about an earlier set of responsibilities and the assumption of obligations. The poor are not embarrassed by this type of economic cycle. They have to compete for a livelihood, and the general pattern of subservience to the rich is handed on from generation to generation.

The author endeavors to reconcile character-

istics of authority and competition in these family relations. On the one hand, there is a pattern of complete subordination and obedience of sons and wives to fathers and husbands; on the other, intense rivalry among the well-to-do families for preferred grave-sites, in the size and decoration of homes, and in various forms of conspicuous consumption. All participate in this competition. Other deviations from ideal patterns are attributed to the circumstances of agricultural and commercial life (p. 262).

The explanation of the decline in prominence of Chinese families in this community seems a little oversimplified. In the latter part of the book, in fact, the author himself points out five types of families instead of two. One includes a very prominent official family whose sons were trained from earliest youth in the care of property and in the responsibilities of their station. The statistical data used to prove the decline of families, taken almost entirely from names mentioned in family, community, and county archives, are perhaps too simple an index to be entirely dependable. Nevertheless, the old American saw of "shirt sleeves to shirt sleeves in three generations" does seem to apply here as well.

In *The Golden Wing* Dr. Lin Yueh-Hwa is concerned essentially with the problem of social equilibrium. He makes use of what he calls an "operational description" of Chinese family life, taking as cases two related families, the Hwangs and the Changs, who reside in a village near Foochow. In a very simple manner he gives a step by step account of the cyclical rise and decline of the fortunes of these two groups as they are influenced by such factors as family solidarity, enterprise, good luck, changes in modes of trade, attack by bandits, law suits, and governmental pressures. Although the book reads more like a novel than a sociological study, it contains valuable detailed descriptions of engagement and marriage, family relationships, business organization and trade practices, the process of rice culture, and funerals.

These formal descriptions, however, are woven around a dramatic life story of the rise of Dunglin from a shiftless young peanut seller and partner of a dealer in drugs to a man of great business ventures. Vivid accounts of methods in law courts, death at the hand of bandits, an unfaithful wife, jealousy among relatives, and failure of old Chang to adjust to the new ways of his enterprising young cousin mark his progress toward eventual decline as an opium-smoking, useless old man who sees the final degrada-

tion of his entire family. Dunglin's cycle ends with the Japanese invasion when he returns to the old farm home from which his high adventures began. This vividly detailed narrative seems far more like a personal family history in its treatment of motives, prejudices, and personality patterns than a fictional account. But it never neglects the cultural framework and controls of the particular community with which it deals.

If the families in the South resemble those in North China, one criticism may be made of both books: the tremendous power and influence of the grandmother is not sufficiently stressed in either. The picture here given of the Chinese woman is one of nearly complete subservience to the male. The reviewer's observations in North China have led him to believe that such subservience is merely a formal pattern in many places, but the role of the grandmother needs further investigation. This suggestion cannot detract from the positive contributions of both of these ably done studies. They will certainly be recognized as significant new contributions to a better understanding of China's family life.

J. STEWART BURGESS

Temple University

The Fortunate Islands: A Pacific Interlude. By WALTER KARIG. New York: Rinehart and Company, Inc., 1948. 226 pp. \$3.75.

Anatomy of Paradise: Hawaii and the Islands of the South Seas. By J. C. FURNAS. New York: William Sloane Associates (In cooperation with the American Institute of Pacific Relations), 1948. 542 pp. \$5.00.

The native peoples of the South Seas, numbering about two and a half million, occupy a combined territory larger than any single nation in Europe, are scattered over an oceanic region equivalent in size to North America, and all are "governed" by foreign powers. The central themes of these two books concern the status of the inhabitants today. Karig focuses on American Micronesia (the central Pacific islands held by the United States under a United Nations trusteeship); Furnas covers the same area plus selected islands in Polynesia and Melanesia—localities ruled by a number of nations. Karig concentrates primarily on illustrating the styles of living while Furnas emphasizes the social consequences of acculturation. Karig is indifferent to the past, to prior studies of the area, and to those groups who deviate from his cultural motif. His portrait

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appears one-dimensional, but within this dimension there is tight integration. Furnas orients the present scene in historic perspective, extracts from past studies relevant raw materials for his analysis, and presents a variety of subcultures. His perspective is multi-dimensional and the thematic ties are more loosely integrated. Both writers are well-known journalists and their books are designed for mass audiences. These two books may prove to be the definitive "popular" accounts of the area. They are more accurate than Willard Price's version of the same area, more current than Yanaihara's and Keesing's studies, and more accessible than the volumes of the U. S. Commercial Company and the German South Seas Expedition.

Neither book contains a sophisticated theoretical framework at a high level of abstraction, yet both possess schemes which determine the kinds of questions asked, the selection of the data offered, and the generalizations made. These merit analysis for they reveal the intellectual processes involved in the fashioning of cultural images of peoples. The conceptual scheme Karig employs for analyzing the social character of Micronesians stems from a familiar construct: *viz.*, the "true" native is stable and balanced, the individual is free of internal conflicts or external constraints, and the culture automatically provides coherent, satisfactory, and ingenious solution to life's needs. Where these conditions do not prevail, the source of the difficulties are construed as stemming from foreign influences. To cite one item: ". . . the health, amiability and resourcefulness of the Micronesians is in inverse proportion to their adaptation to Occidental influences." Acculturation is deemed, therefore, as an "unfortunate" social process. "Let's leave one corner of the earth uncontaminated by tin-can civilization." Subordination of the native peoples to foreign control is accepted as inevitable and its effects should be minimized by maximizing self-autonomy. The component features of these precepts are not analyzed nor compared with alternative theories held by many native groups in the islands and by foreigners who have discussed the same area or problem. A wide assortment of case materials is presented which lend themselves to other interpretations.

Furnas offers a series of case histories and, on the basis of these, constructs a series of generalized types. The criteria employed in the type construction are not specified but may be inferred to be threefold: the amity between the two status groups, the degree of "successful"

accommodation of the subordinate society while undergoing selective cultural assimilation, and the managerial abilities of the superordinates. The existence of a hierachal order is posited on the moot proposition that there is a fundamental difference in the core qualities of the dominant western and the dominated South Seas societies. Within this framework, the author offers a provocative analysis of the role and value systems of the social scientist dealing with cross-cultural relationships.

The two books may have a dual interest: as source materials on a sector of the world heretofore known to most sociologists largely in terms of personality-culture studies, and as case materials for the study of the cultural constructs of mass media writers in their search for understanding of the larger world.

JOHN USEEM

University of Wisconsin

Suomen Lappalaiset Vuoteen 1945 [The Finland Lapps to the Year 1945]. By T. I. ITKONEN. Helsinki: Söderström, 1948. 2 vols., 589 and 629 pp. 1950 Markke.

The Lapps of northern Europe have long attracted widespread interest. Perhaps it was their unswerving devotion to the ways of peace, a quality not always observable among Europeans, that won them such attention. Again, the appeal may have been other unique features of Lappish culture. Or was it the fact that these people could be reached and studied with relative ease? Whatever the reason, the literature on the Lapps has grown to immense proportions in the years since 1673 when an Upsala professor, Johannes Schefferus, published his pioneering study, *Lapponia*. Much of the output has been fictional in form, some of it of excellent quality; but there is also a solid body of scientific research. Regional studies began to appear early. In 1767 K. Leem published a study of the Norwegian Lapps, *Beskrivelse over Finmarkens Lapper*; the Lapps of Sweden were examined by G. von Düben in a detailed work, *Om Lappland och Lapparne: företradesvis de svenska* (1873); and in 1890 an excellent account of the Russian Lapps became available, Nikolai Haruzin's *Russkie lopari*.

Publications on the Finland Lapps number about 500 titles, the major contributions having been made by such scholars and writers as Isak and Jacob Fellman, Samuli Paulaharju, Väinö Tanner, and E. A. Virtanen. Recent works in languages other than Finnish include Norah Gourlie, *A Winter with the Finnish Lapps* (Lon-

don, 1939), H. Sutherland, *Lapland Journey* (London, 1939), Jean Gabus, *Sous les tentes laponnes* (Paris, 1937), and L. Kattwinkel, *Lapin hulla: Eine Winterfahrt durch lappische Wildmarken* (Berlin, 1942). It has remained for Dr. Itkonen, however, to prepare a truly definitive study. No aspect of Lappish culture seems to have been neglected in this work of encyclopaedic proportions. There are 39 chapters, among which are found such headings as geographical setting, history, anthropology, language and names, character traits, buildings, trade, fishing, hunting, agriculture, care of deer and dogs, customs and myths, household furnishings, and bodily ornaments. In a word, its scope ranges from birth to burial rites, from mundane toil to happy moments of recreation.

The amount of research represented by the two volumes is staggering. The bibliography lists nearly 350 titles (of which Itkonen has authored 35), covering the chief Finnish published and archival sources, as well as the significant Norwegian, Swedish, and German monographs. Some dozen field investigations were carried on by the author during the period 1913-1947. Scholars will not complain of a paucity of documentation: the chapter on settlement, for instance, has no less than 246 footnote references by a hasty count. Illustrative materials, too, are abundant; there are 442 well-chosen photographs and drawings, and 15 maps.

Itkonen's study of the Finland Lapps has been described in the country of its publication as "one of the most important achievements of recent Finnish scholarship." This is not extravagant praise. No scholar whose interests touch the Finland Lapps can have an easy conscience until he has put his hands on this monumental work.

JOHN ILMARI KOLEHMAINEN
Heidelberg College

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Social Denmark: A Survey of the Danish Social Legislation (Second edition). Translated by W. E. CALVERT. Copenhagen: Socialt Tidsskrift; New York: Crown Publishers, 1947. xv, 475 pp. 20 Kr., \$6.00.

Denmark has long been numbered among the nations most advanced in social legislation, but surprisingly little about the details of Danish provisions has been published in English. To celebrate the fiftieth anniversary in 1941 of the beginning of modern Danish social legislation, the Amalgamated Trade Unions and the Danish Employers Association subsidized a memorial volume to be prepared by members

of the Ministry of Labor and Social Affairs. With the occupation in 1940, the projected volume was pointed somewhat toward convincing Germans that Danish social legislation was better than theirs, at least for Denmark, and a German translation was published shortly before the Danish edition appeared in 1941. The original title was *Danish Social Legislation, 1891-1941*. With some revision and the addition of an introduction by Kjeld Philip, professor of economics at the University of Aarhus, the English translation was issued in 1945 and reprinted in 1947.

The brief introduction describes the social-occupational structure of Denmark and sketches the historical development and general characteristics of social legislation. The longest chapters are devoted to social insurance (sickness, old age, invalidity, workmen's compensation, unemployment), public assistance, and organization of the labor market. There are chapters also on hospitals and the prevention of disease, education, recreation, and housing. The final chapter on cooperatives is justifiably short, since the cooperative movement has been treated at length in other books in English. The bibliography is arranged by chapters. References are mainly to Danish, German, and Swedish publications, but a few English titles are listed. The book is attractively illustrated with photographs and spot maps, but a detailed table of contents only partially makes up for the lack of an index.

Social Denmark, like Alva Myrdal's *Nation and Family* (1941), is offered to the English-speaking world as information about a Scandinavian democracy's program of social security. Though conditions and legislation in Denmark and Sweden are quite similar, these two books reflect the differences in their preparation. Myrdal's book is firmly value-oriented, and the various kinds of Swedish social legislation are related to the birth rate and family life. She criticizes the inadequacies of existing provisions and indicates the difficulties and the necessary compromises of principle in enacting recommended measures. By contrast, *Social Denmark* appears as the large and costly propaganda information bulletin it is. It confines itself to outlines of formal legal structure and mention of earlier legislation in each field, with asides about the superiority of these provisions for Denmark. Presentation is topical and integration attempted only in a superimposed introduction. The few statistics are limited to the number of people covered by a given law and

the number receiving benefits. There is no indication of the difficulties encountered and the compromises necessary in initiating and changing the laws. Defects of the current structure—e.g. duplication of facilities and inequalities of treatment due to the complexity and patchwork character of the system, even after its 1933 codification—receive less than passing attention. These are not so much criticisms, however, as an indication of the almost inevitable effect of the book's purpose and history on its content, a warning of what the reader must not expect to find. Description of the actual legislation is as detailed as it could be without bogging down in legalisms. Finally, it is at present the most extensive account in English of the social legislation of any of the Scandinavian countries.

RUTH RIEMER

University of California at Los Angeles

Die Kommunistische Partei Deutschlands in der Weimarer Republik. By OSSIP K. FLECHTHEIM. Offenbach A.M.: Bollwerk-Verlag Karl Drott, 1948. xvi, 295 pp. No price indicated.

At the present time it is a distinctive achievement to write a critical analysis of Communism without becoming a violent antagonist or protagonist. Professor Flechtheim's contribution to the literature is written from the point of the humanistic socialist. He has traced the development of the KPD in the Weimar Republic from its emergence during the first World War to its destruction by the Nazis in 1933. His analysis relies on published statements by the leaders of the German labor movement and in particular on official publications of the Communist Party. On the basis of this material the author gives a detailed recital of the changing party line, the factional struggles, the alternating dogmatism and opportunism of the party's political actions and orientation. Of special interest to sociologists will be the data on party membership and its social composition, the discussion of labor aristocracy, and the concluding appraisal of the relation between ideological struggles in the party and the changing internal and international environment of the Republic. This reviewer regrets that the author did not devote more space to these topics, although it is clear that evidence in these fields is difficult to come by.

A comprehensive study which is as good as this one can be criticized for not being perfect. In covering the Communist Party literature with thoroughness and discrimination Dr. Flechtheim had the force to neglect important seg-

ments of the story on which he might otherwise have dwelt. He does not draw on the voluminous literature of the social democrats nor does he deal with the parliamentary record of the KPD, although this would have added to an understanding of the Communist Party. He omits mention of some important sources as well, such as Paul Levi's *Unser Weg*, Dännig's *Der Arbeiterrat* and *Das Neue Russland*. And in his analysis of Communist Party membership he could have made advantageous use of Theodore Geiger's analysis of the German social structure.

Any author must in some measure be arbitrary in his selection of source materials. But it is relevant to ask how far the perspective an author has chosen will serve inadvertently to reveal or hide certain aspects of his problem. Repeatedly Flechtheim quotes Communist leaders as complaining that their party made no headway among the industrial workers. But his sources do not reveal that the regulations governing labor relations were so written as to exclude Communist trade unions from representing workers before the labor courts. He fails to mention many other legal liabilities (especially criminal) which hampered the work of the Communists particularly. Similarly, the author presupposes that political parties may be studied in terms of the internal controversies over their immediate political objectives and their long-run ideological orientation. That in turn presupposes that the history of a political party is more or less identical with the history of the conflicts within its ruling minority. Perhaps he underrates the fact that Russian influence and assistance were important reasons for the characteristics of the Communist oligarchy and its internal struggles. The author suggests in his concluding chapter that these doctrinal struggles within the ruling minority were so many responses to the changing historical situation as this affected both leaders and members of the party. But I think he could have gone further in integrating his analytic perspective with his chronological narrative in this respect.

It is to be hoped that Dr. Flechtheim's study will be translated and published in this country. As conscientious a study as this is certainly worthy of attention at this time in view of so much haphazard writing in this field. It might be suggested that the addition of an index and subheadings in the chapters would aid the reader considerably. The volume contains numerous quotations from original materials which

would be difficult of access otherwise. The book may be obtained directly from the author at Colby College in Waterville, Maine.

REINHARD BENDIX

University of California

Personality and Culture in Eastern European Politics. By DINKO TOMASIC. New York: George W. Stewart, Inc., 1948. 249 pp. \$3.75.

This study undertakes to explain the political instability, the hegemony of the military, and the dictatorial aspects characteristic of the regimes since the Middle Ages in the area between the Baltic and the Mediterranean. The specific countries include Yugoslavia, Bulgaria, Albania, Romania, Hungary, Czechoslovakia, Poland and, incidentally, Greece. The author, now a sociologist at the University of Indiana, recognizes the part that successive religious and imperialistic conflicts have played in this instability, but he sees "the interrelations of the three basic structural elements of the countries of Eastern Europe—the herdsmen, the peasantry, and the urban strata" as factors of primary importance for understanding events in this area. He analyzes the herdsman's culture ("Dinaric") and the culture of the peasants ("Zadruga"), and shows how the direct impact of Dinaric society on the urban society resulted in political hegemony for the former and instability for all.

The interpretative emphasis in this study rests upon the types of personality which psychogenic influences in the family (here rooted in the general culture) produce. Utilizing ethnographic materials, and some historical data, the author concludes that an affectionally-rejecting culture produces "power-seeking" personalities, while an affectionally-satisfying culture produces "emotionally well-balanced" and "power-indifferent" personalities. As an essentially psychiatric explanation of politics in this area, rather than one in terms of balance of power or such symptom-phenomena as the megalomania of a particular potentate, this study is an outstanding and distinctive contribution to social science theory as well as to an understanding of this area. While it contains a few minor negative aspects, the study is, on the whole, an invaluable combination of the psychiatric and cultural approaches in the analysis of literate societies relatively little known in the West.

Though coming from Yugoslavia, where he was on the staff of the University of Zagreb, the author demonstrates a thorough acquaintance with American sociology and a flawless literary style. This work—so very readable—

should prove eminently useful not only to the cultural anthropologist or sociologist, but also to the social psychologist and social psychiatrist. It should also prove valuable to those handling "area courses" in the East European field, to those interested in theories of leadership, and to statesmen who attempt to establish peace and freedom in the "powder-keg" of Europe.

STEPHEN W. MAMCHUR

Wayne University

Politics Among Nations: The Struggle for Power and Peace. By HANS J. MORGENTHAU. New York: Alfred A. Knopf, Inc., 1948. xvi, 489, xix pp. \$5.50 (Text edition, \$4.25).

In a reaction against an older intellectual tradition sociologists have turned aside from the doings of chancellors and diplomats and devoted most of their research to the study of individuals and groups below their own status. The sources and centers of power in modern society have been very largely ignored by members of our guild. They will be the losers if on this account they overlook the contribution of this intellectually cosmopolitan political scientist.

Politics Among Nations is an attempt to reduce the behavior of the largest social aggregates, modern nation-states, to recognizable order. The author avoids the temptation to be seduced by the present, and reveals at several points a firm grasp of the facts of historical growth and change. The central theoretical concept is the balance of power. According to the author's interpretation of this concept, in international politics automatic forces come into play as soon as any state attempts to expand its power. These forces work in the direction of a restoration of equilibrium in the international distribution of power. Thus international relations are characterized by a struggle for power among a series of approximately equal coalitions.

The stability that is achieved in this fashion "is always in danger of being disturbed and, therefore, is always in need of being reestablished" (p. 131). In the period following the Second World War this precarious equilibrium has undergone significant structural changes, which the author points out in clear and cogent fashion. A subsidiary yet extremely valuable portion of the argument analyzes the part played by a common aristocratic culture in the conduct of international relations and the consequences of the disappearance of this factor in modern times.

In the chapter on the evaluation of national power and the intellectual challenge involved in this task, the major fallacies in most popular and much official thinking on this topic are neatly brought to the surface. In general the analysis of various popular panaceas is of a very high order. As the arguments cannot be examined here, the curious sociologist will be rewarded—if not necessarily convinced—if he reads for himself the author's corrosive interpretations of world public opinion, public diplomacy, the international police force, the world state, the United Nations, UNESCO, and proposals for peace based upon "mutual understanding."

In the reviewer's opinion the major weakness of this study lies in its shaky psychological underpinning. With no empirical evidence beyond the questionable parallel with animal societies, the author assumes that the drive for power is both strong and universal. The occasional concessions to social conditioning play no part in the argument. On p. 199 the author tells us that "All human beings seek power, and hence, seek social distinctions, again varying with the particular pattern of their culture, which put them ahead of and above their fellow men." Does no one have greatness thrust upon him? And what may be the political significance of the alleged fact that culture may modify and channel the drive for power? It is perhaps in this respect that the limitations of "realistic" analysis, which refrains from a scientific exploration of possibilities and sticks to the record of past actualities, become clearest. Assuming the existence of an instinctive power drive, the author has to engage in considerable logical gymnastics to show how this power drive becomes transferred from the rank and file of the citizenry to the arena of world politics. This is a very difficult point and one that deserves additional research and reflection. Finally, many readers will take issue with the very thin speculations on national character, though this reviewer finds them no worse and no better than more fashionable attempts to explain diplomacy by toilet training. Something may yet come from speculation and research along these lines. From the reviewer's standpoint these weaknesses boil down to an under-estimation of the variety and plasticity of human behavior.

Social scientists trained to question authority will probably be irritated by the author's device of substituting an apt quotation—preferably from an author dead at least a hundred years—for rigorous proof. Although the scientific evi-

dence on the effectiveness of totalitarian propaganda may leave much to be desired, a phrase from Edmund Burke is scarcely an adequate substitute.

None of these strictures concerning the psychological groundwork or the use of evidence are altogether crucial. At most points the author's arguments will stand or fall on the basis of his interpretations of the institutional mechanisms of world politics, such as the balance of power. Morgenthau's book is that rarity, an attempt to face crucial contemporary problems in the scientific spirit. Its achievements stand head and shoulders above the crusading or pontifical writings that constitute the major intellectual fare in this field.

BARRINGTON MOORE, JR.

Harvard University

The World Community. Edited by QUINCY WRIGHT. Chicago: The University of Chicago Press, 1948. x, 323 pp. \$5.00.

This is the report of a conference called by the Harris Memorial Foundation, in March 1947, at Chicago. In order that the entire time might be devoted to discussion, the committee in charge first drew up a "Preliminary Memorandum" setting forth some of the basic issues and then asked six persons to prepare statements on six aspects of the central theme. All this material was distributed in advance to the forty-three participants. The result is a collection of wide-ranging observations—some obscure, some irrelevant, some pointless, but many pointed and instructive. The whole lacks system, in spite of careful planning. However, the purpose was not to furnish statesmen with formulae for policies or plans of action, but to clarify academic opinion. In its attempt to achieve this aim the conference can claim only a partial success.

The six papers, which cover a total of only eighty pages, were: "World Community, World Society, and World Government," by Louis Wirth; "World Culture," by Margaret Mead; "World Economic Contacts and Economic Policies," by Kenneth C. Boulding; "International Communication," by Robert C. Angell; "World Loyalty," by Harold D. Lasswell; and "World Institutions," by Pitman B. Potter. Thus there remain over 200 pages of "discussion" in the course of which a wide variety of matters are touched upon, usually briefly and somewhat jumbled, as would be expected in such a general, free-for-all reaction of diverse minds in spontaneous combustion. This is, in fact, an excellent

example of a group of academicians from different but related disciplines trying to convey meanings to each other. The reader can share to some extent the experience of the participants, for he will almost certainly find passages which fail to convey any intelligible meaning to him.

The committee on arrangements seems to have anticipated some such difficulty and thus gave Professor Wirth the job of clarifying terms at the very start. In spite of heroic efforts he seems to have failed. A small committee afterwards wrestled with the same problem, but various speakers insisted on giving their own definitions. In any case the reader will share frustrations with participants at the end of nearly fifty pages. On the subsequent topics there were numerous high points. Miss Mead set up cultural diversity as a supreme value in any realistic and desirable world civilization, and found wide agreement. There was no indication, however, of how much diversity is either desirable or feasible, nor how to achieve it, if the world is to be unified.

On the whole the outlook seemed to most of the discussants to be none too hopeful. Some found international equilibrium destroyed and unlikely to be restored soon. Neither economic trends nor varied and expanding forms of communication seem certain to develop a wider sense of mutual responsibility and agreement. Even international institutions, which constitute a main hope, are in danger of being used for selfish nationalistic purposes. Political interests tend to permeate not only economic but even cultural exchanges. Mrs. Wright even expressed the view that the amount, variety, and complexity of information needed to form an intelligent opinion regarding world affairs would be beyond the mastery of any average, or even better than average, individual.

One can understand why one participant says: "As the conference progressed, I knew less and less what people were talking about" (p. 302). Another speaks of "a lot of bunk" (p. 277); one confesses "my head is reeling. I am going home with a cultural Katzenjammer." (p. 305). In other words, failure to recognize from the first the bipolar nature of the present world situation reduced much of this discussion to little more than the airing of opinions in a vacuum. At one point Professor Parsons called for "some discussion of the specific facts of the Russian system in its specific relations to our system" (p. 281), but only on page 302, the last of the discussions, do we get the observation from Professor Morgenthau that, even if

we know all about Russian psychology and institutions, we would still be in danger of conflict if the Russians should want what we do not wish to give up.

There is an index, evidently hastily done. From it one learns that Donald Taft and Robert A. Taft are the same and that Frank Notestein has changed his name to Philip.

FRANK H. HANKINS

Smith College

The Political Community: A Study of Anomie.
By SEBASTIAN DE GRAZIA. Chicago: The University of Chicago Press, 1948. xx, 258 pp.
\$4.00.

Few subjects are more pervasive in contemporary thought than that of insecurity. In literature, theology, psychology, and the social sciences, themes of dissociation and decay are matched by those stressing the moral isolation of the individual. Every age has its literature of regeneration; our own is directed not to the ancient quest of man for higher virtue but to the almost obsessive desire to find security in present circumstances. It enjoins not morality but adjustment.

Much of the striking quality of De Grazia's work lies in its insistence that psychological security and a transcendent morality are inseparable. Like the post-Revolutionary Traditionalists in France, he finds the main problem of our society to lie in the conditions created by revolution, individualism, and secularism, forces which have emptied society of its moral content and left individuals bewildered and purposeless.

In anomie, he has chosen a theme from Durkheim, elaborated it, and added, first, a theory of its psychological roots (which he finds in the child's dependence upon parents) and, second, a political orientation that stems from Plato and Rousseau. Normally, political and religious systems of belief reinforce those of family, giving to the individual his sense of status. But in modern society a distractive process interposes itself—Capitalism. Capitalism has created the dynamic of competition, to which Protestantism has joined the powerful ethic of activism. The result is a conflict of "the belief system of family, state, and religion with the capitalist ideology in respect to their directive concerning treatment of members of the community."

Against this background the author develops and documents his diagnosis of the anomie community. There is "simple anomie," which is the outcome of conflicts among beliefs, and "acute

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anomie" which is described as "the disappearance of order and rules, through the degeneration of the ruler" (political, economic, or religious). Typical adaptations to the former are reflected in the widespread quest for affection—in work, romance, and interest associations. Acute anomie produces more drastic adaptations, both individual (suicide and mental disorder) and collective (mass movements). The author's indictment of contemporary society is severe.

If we would rid ourselves of the anomic community we must restore the true political community. This can be done only by abolishing the competitive directive ("Man is a creature who must avoid competing loyalties"), and by re-creating a sense of the ruler ("To be rulerless is to be ruleless, to be ruleless is to be anomie"). The political bond must become exalted, for "to be freed of separation-anxiety is the citizen's bliss."

This bare outline can give no hint of the prophetic tone of the book, the diversity of citation and frequently valuable notes, nor of the occasional penetrating insights. Particularly praiseworthy are the treatments of anomie in relation to unemployment, war, and mass movements. Since it is in the prophetic rather than the rationalist tradition, criticisms may be gratuitous. Nevertheless, I venture a few. There is the author's implicit assumption that a symbolic or ideological continuum is extendable to institutional relationships. Thus the psychic dependence of child upon parent becomes, for De Grazia, the adult's dependence upon church

and state, and we are told that "political and religious systems can never fundamentally conflict. Both of them are descended from the community of the family." But within any determinable historical perspective such systems neither necessarily evolve from the family nor endure without conflict. See the histories of Greek, Roman, and modern European polity. The treatment of associations is unsatisfactory, and misses the striking diversity of associativeness among socio-economic strata. To categorize voluntary associations simply as vicarious experiences for the frustrated is questionable and, in any case, overlooks the important distinction between personal motive and the functional part of associations in a complex civilization. Were the author less devoted to the state as the true area of adult security, he might have given more attention to trade unions and professions and less to booster clubs. Finally, the nature of his argument prevents him from considering the possibility that the political relationship itself has been one of the strongest acids on the social fabric. Durkheim sensed this; so did Le Play, de Tocqueville, and von Gierke. But De Grazia, with a hostile eye on the pluralists, refuses to differentiate between state and social organization. He hints at a succeeding volume in the tradition of Plato. One looks forward to it but with the hope that his pen will draw also from the analyses of the Platonic community made by Barker, Kelsen, and, more recently, Popper.

ROBERT A. NISBET

University of California

BOOK NOTES

Cultural Sociology (A Revision of An Introduction to Sociology). By JOHN LEWIS GILLIN and JOHN PHILIP GILLIN. New York: The Macmillan Company, 1948. viii, 844 pp. \$5.00.

A new title, a more logical arrangement of chapters dealing with social dynamics and the individual in society, a total of 34 additional pages of text, and eight more cartoons from *The New Yorker* constitute the principal features of the revision. The title aims "to give more explicit recognition to the contributions from both social anthropology and sociology." Whether or not title-changing achieves such a purpose, the hybrid vigor of this joint product of the Gillins, *père et fils*, testifies to their con-

siderable success in blending an impressive amount of old and new material from both disciplines. If its very scope may be detrimental to its tidiness of form, all credit is due the authors for their efforts in behalf of furthering positive integration of the social sciences. Provided that they have access to a library of the first rank, instructors and perhaps even their students using this text will be grateful for the voluminous footnoted suggestions to "see" a host of more recondite sources for further discussion of points raised; this book is surely more than an "introduction" in the usual sense. Some additional items have been included among the "Readings" and "Exercises," which march with each chapter in the honored tradition of textbook construction.

Culture and Society. By G. S. GHURYE. Bombay: Oxford University Press, 1947 [1948]. 237 pp. \$8.00.

Despite its seemingly straightforward title, this book is conceived in a mood of philosophical eclecticism and discusses ways and means for creating civilization and culture and for maintaining them at high levels. Civilization is defined as the sum total of the social heritage projected onto the social plane; culture is the same heritage focussed on the individual plane. The author recommends a planned society which will make special provision for "the development and status" of universities. A curious disregard for technical distinctions and shades of meaning which may be carried by such English words as "culture" produces interesting, if blurred, effects. Dr. Ghurye heads the sociology department at the University of Bombay and is the editor of the University of Bombay Publications, Sociology Series, of which the present volume is the first.

Social Forces in Southeast Asia. By CORA DU BOIS. Minneapolis: University of Minnesota Press, 1949. 78 pp. \$2.00.

A series of lectures on the peoples of contemporary Southeast Asia. Dr. Du Bois first outlines the concepts of cultural anthropology which she uses and then proceeds to sketch broadly the pre-war culture of Southeast Asia on the basis of which she analyses current trends and possible future developments. Though brief, the volume is a good introduction to the subject, and an excellent commentary on the nature of area studies in general.

The Uniform of Colour: A Study of White-Black Relationships in Swaziland. By HILDA KUPER. Johannesburg: Witwatersrand University Press, 1947. xii, 160 pp. 15s.

This detailed description of culture contact and processes of adjustment between the 150,000 Swazi and 3,000 Europeans in the High Commission Territory of Swaziland ably supplements the same author's penetrating analysis of rank and stratification in tribal Swazi society, *An African Aristocracy*. Here she draws upon additional materials gathered during her field work between 1934 and 1937 to show how European domination has affected traditional Swazi behavior and values. "The present Swazi society is not simply the continuation of the traditional society with certain powers taken from the chiefs and others bestowed on them; nor can it be described in terms of clear

cleavage, or segregation from the white group. A new society has resulted . . ." (pp. 99-100). Nevertheless, "throughout South Africa colour has become the primary index of status in all activities. A small self-conscious white community possessed of the technology of a great mechanical age lives in vital dependence on the labour of a black subject people" (p. 26). Cleavages in both groups, as well as the chasm separating blacks and whites, are carefully described and documented; "racial" theories, economic life, and native policies—in labor, health, and education—are analyzed; and the roles of administrators, missionaries, traders and recruiters are shown in concrete detail. While the adaptability of the Swazi to new life conditions has limited conflict and open violence between the two societies, interdependence does not automatically end hostilities. Like many other Bantu groups, the Swazi yet remain a disadvantaged peasantry who are victimized by forces over which they have virtually no effective control. Eschewing prophecy and preachments, Dr. Kuper's case studies of the Swazi give an evaluative insight into some of the basic ingredients of modern South Africa which has great practical and theoretical significance.

Job Horizons: A Study of Job Satisfaction and Labor Mobility. By LLOYD G. REYNOLDS and JOSEPH SHISTER. New York: Harper and Brothers, 1949. x, 102 pp. \$2.25.

This short but rewarding book attempts, through interviews with 800 workers in "a medium-sized New England manufacturing center," to answer three questions: how do workers find and select jobs? why do they keep or quit them? and what do they think of occupational advancement? An appended note on methods lends weight to conclusions which may interest sociologists and surprise economists. In general, the replies of the workers interviewed tend to undermine economic theories based on rationality and freedom of choice and cast strong doubts on motivations often considered crucial in determining "economic behavior." The authors call their book a preliminary report addressed primarily to the lay reader. It is their intention to follow it with a larger work in about a year's time.

Crown Zellerbach and the Pacific Coast Pulp and Paper Industry: A Case Study. By CLARK KERR and ROGER RANDALL. Washington, D.C.: National Planning Association, 1948. 96 pp. \$1.00.

First in a proposed series of fifteen studies dealing with "causes of industrial peace," this report should interest students of industrial sociology who are concerned with the successes, as well as the failures, of collective bargaining. The authors assess various factors which seem relevant to what is apparently an outstanding case of union-management cooperation, e.g. the economic and social environment of the industry, and the attitudes and practices of management and union leaders. One might wish for more information on the attitudes of workers themselves, but this may be asking too much of so brief a study.

Politics and Poverty. By LEWIS C. ORD. New York: Funk and Wagnalls Company, 1948. 188 pp. \$3.00.

The author, an industrial consultant with practical experience in Canada, the United States, and Great Britain, undertakes to show why real wages of British workers are low and what may be done to remedy this situation. While sociologists may find the material of secondary interest and may disagree with the conclusions, they will appreciate the presentation of the problem as a social, as well as an industrial, one. Ord believes that industry should be organized from the bottom up—foremen should service workers, supervisors foremen, and so on. Top heavy management, especially when surmounted by governmental bureaucracy, is to be avoided. Government should stay out of business, but should actively promote free and fair competition.

Flophouse. By JESSE WALTER DEES, JR. Manchester, New Hampshire: Marshall Jones Company, 1948. xxv, 170 pp. \$4.00.

This addition to the sparse literature on the homeless man summarizes several earlier works in the field as background for a fifty page report of the author's own field experience as a participant observer. A suggestive if inadequately developed thesis concludes that, in order to avoid the stifling effects of "shelterization," outdoor relief should be substituted for the prevailing system of regimented relief in public shelters. Despite a certain redundancy and lack of organization, this book should provide useful supplementary reading in the field of social maladjustment.

The Open Self. By CHARLES MORRIS. New York: Prentice-Hall, Inc., 1948. xi, 179 pp. \$3.00.

Another attempt by a philosopher to integrate what is known of the somatic, psychic, and social aspects of personality development and adaptation in modern society. The synthesis represents a novel and peculiar blend; it merges Sheldon's emphasis on constitutional differences with Freud's stress on unconscious psychic determinants, all heavily weighted with semantic, metaphysical, religious, and moral connotations. Except for its point of view, and forceful to flippant style of writing, the book presents relatively little that is new or notable.

Diagrams of the Unconscious: Handwriting and Personality in Measurement. Experiment and Analysis. By WERNER WOLFF. New York: Grune and Stratton, 1948. xiv, 423 pp. \$8.00.

The first part of this volume, entitled "Form and Movement," tries to demonstrate the existence of various consistencies of size, form, etc., in the handwriting of individuals. The second part, "Expression and Movement," deals principally with the meaning of these consistencies in relation to the personality of the individual. With few exceptions, the approach throughout is illustrative, the author's statements being supported by examples rather than by more conclusive evidence. Many signatures and other handwritten materials, largely from well-known persons and intriguing in their own right, are reproduced and discussed. But loose organization, lack of clearly formulated evidence, and a dearth of conclusions leaves graphology about where it was before the appearance of this book.

Film and Education: A Symposium on the Role of the Film in the Field of Education. Edited by GODFREY M. ELLIOTT. New York: Philosophical Library, 1948. xi, 597 pp. \$7.50.

This volume should be of most use to educational sociologists. Its thirty-seven chapters include analyses of films, their applications both inside the classroom and out, as well as problems of administration. There is also informative comparative material. Because so many men have contributed to it, the volume suffers from repetition and there is some unevenness in presentation. Nevertheless, the general impression is favorable.

Scientists and Amateurs: A History of the Royal Society. By DOROTHY STIMSON. New York: Henry Schuman, 1948. xiii, 270 pp. \$4.00.

A laudatory history of the British institution which, more than any other, deserves recognition as the initiator of organized scientific research in modern times. The beginnings of the Royal Society, especially its first quarter century, rightly receive extensive treatment. The interest of the Society's best minds during this "classical" period in practical theory—in "knowledge as it hath a tendency to use"—is made clear. By contrast the ups and downs in following centuries of policies and finances, membership and housing, tend to crowd the space devoted to scientific accomplishments of its members. In her exclusive focus on natural science matters, Dr. Stimson may disappoint social scientists who expect at least passing reference to the pioneer demographic work of Graunt, Petty, and Halley, even in such a brief and general account as this.

Cybernetics, Or Control and Communication in The Animal and The Machine. By NORBERT WIENER. New York: The Technology Press (John Wiley and Sons, Inc.), 1948. 194 pp. \$3.00.

In some ways this is an impressive book. The author is erudite: he ranges from problems of Newtonian Time and statistical mechanics to ideas about psychopathology and human society.

In the process he resorts to mathematical formulae, analogies, conjectures, hypotheses, and anecdotes. Yet in the end it is doubtful if social scientists have much to learn here.

The central thesis seems to be that certain automatic mechanisms, of an electronic variety, are remarkably similar to the human nervous system. From this it is deduced that students of man and society can profit from the investigations of communication engineers and electronic specialists. The new science of "Cybernetics" (Greek *steersman*), if rigorously pursued, will unlock many human, animal, and physical problems.

It may all be true, but the author's admitted unfamiliarity with the social sciences, and his dependence upon analogical reasoning, will leave many social scientists sceptical. The main difficulty with the book is that it is both too technical and too naive: too technical as regards Professor Wiener's native fields, and too naive as regards man and society. Experts in the former may be impressed with speculations about the latter, but social scientists will be more cautious. The book also suffers from a rambling style, which obscures many points, and a clumsy typographical job, which is often irritating.

Courtship and Marriage

By Francis E. Merrill, Dartmouth College

★ This is primarily a study of the vital sequence of relationships preceding and following matrimony and the discussion centers on these relations rather than on the institutional aspects of the family. The author's major premise is that courtship and marriage are *social* relationships; his minor premises are, *first* that courtship in America is influenced to a unique degree by romantic love, and *second* that every marriage is broadly determined by the roles which the principal participants play therein.

Approx. 320 pp.

Probable Price \$2.75

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